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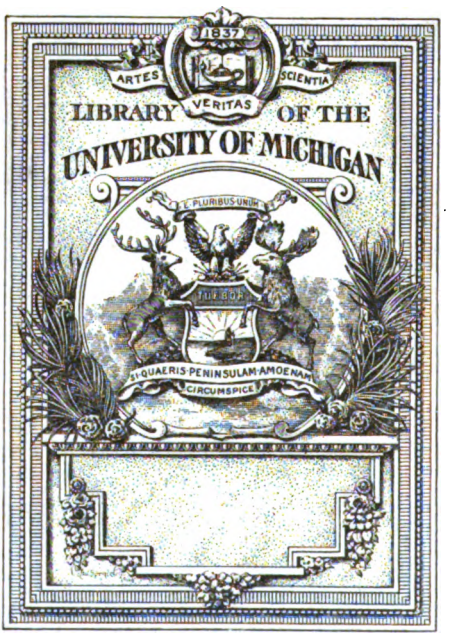

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PRESENTED BY
THE ROYAL ASTRONOMICAL SOCIETY
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MONTHLY NOTICES

OF THE

408:2

ROYAL ASTRONOMICAL SOCIETY,

CONTAINING

PAPERS,

ABSTRACTS OF PAPERS,

AND

REPORTS OF THE PROCEEDINGS

OF

THE SOCIETY,

FROM NOVEMBER 1877 *TO NOVEMBER* 1878.

VOL. XXXVIII.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. *November* 9, 1877. No. 1.

WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

Robert C. Carrington, Esq., Indian Marine Survey, Calcutta;

Alexander Davidson, Esq., Addison Lodge, Ridgway Place, Wimbledon;

Thomas Gregory, Esq., Merchants' College, Blackpool;

Lieut. F. W. Jarrad, R.N., Deputy Superintendent, Indian Marine Survey; and

Dr. A. K. Rollit, LL.B., B.A., Sheriff of Hull, President of the Royal Institution, Hull, 18 Trinity House Lane, Hull;

were balloted for and duly elected Fellows of the Society.

Mr. David Gill's Expedition to Ascension; Correspondence and Reports.

Downing Street, August 20, 1877.

MY LORD,—I am directed by the Earl of Carnarvon to transmit to you, with reference to your letter to Mr. Lowther of the 9th June, a copy of a despatch from the Governor of St. Helena relating to the possibility of taking astronomical observations on that island.

I am, my Lord, your Lordship's obedient servant,

JOHN BRAMSTON.

Lord Lindsay, M.P.

Governor Janisch to the Earl of Carnarvon.

(COPY.)

[No. 33.] *St. Helena, July* 7, 1877.

MY LORD,—I have the honour to report that Mr. D. Gill, referred to in your lordship's despatch No. 73, arrived at this island on the 1st instant, and it gave me much pleasure to afford him all the assistance I could.

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2. It is a matter of great regret to me that his arrangements require him to proceed to Ascension for the proposed observations, instead of making them at St. Helena, the position of which is so perfectly adapted to the observation of *Mars* in its present southern declination.

3. Mr. Gill himself shares in this regret, and informs me that it was at first intended to select St. Helena for the purpose, and that the alteration in the plan was caused by a mistaken supposition that the atmosphere of this island was too cloudy to be favourable. This opinion was founded upon published meteorological observations made at Longwood; and it was not until his arrival here that Mr. Gill became aware, from his own observations, that the clouds which prevail at Longwood and other elevated positions to windward do not usually extend to James' Town and the leeward side of the island.

4. I have mentioned this circumstance in the hope that, looking to the approaching transit of *Venus*, a similar mistake may not prevent astronomers from availing themselves of the advantages offered by the position of this island.

I have &c.,

(Signed) HUD. R. JANISCH.

Right Honorable the Earl of Carnarvon, &c. &c. &c.

Mr. Gill's Reports: Preliminary Report, dated August 15, 1877.*

I beg to forward to you, as President of the Royal Astronomical Society, the following report of my doings up to the present date.

Accompanied by my wife, I left London on the 14th June, taking with me the chronometers of the expedition, received the same day from Greenwich, and the following day at noon sailed from Dartmouth, and reached St. Helena on the morning of Sunday, the 1st July. Instruments and baggage were safely landed; and, thanks to a despatch from Lord Carnarvon to the Governor, procured through the kindness of Lord Lindsay, comfortable lodgings were ready for us, and accommodation for the more delicate instruments was provided in the Castle. The heavy packages, houses, &c., were stored by Messrs. Solomon, Moss, Gideon, & Co., agents for the Donald Currie line of steamers. Captain S. T. Olliver, R.A., kindly offered the services of some of his gunners to carry such instruments as I desired to the site of Johnstone's Observatory (Ladder Hill)—now the Artillery and Engineer mess-room; and, as I wished to make a chronometric connection between Ascension and St. Helena, I gladly availed myself of this offer. On Monday (July 2) the chronometers and reflecting circle were conveyed there; and altitudes of the Sun,

* To prevent a possible misunderstanding of expressions in these Reports, it is right to mention that the Expedition was undertaken only in part at the costs of the Royal Astronomical Society.—ED.

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morning and evening, were observed for time. Similar observations were repeated frequently during my stay on the island; the chronometers were wound and intercompared daily.

During my stay on the island, I visited Halley's Mount, and discovered what undoubtedly has been the site of Halley's Observatory. An oriented foundation of a building still exists, overgrown by ironwood scrub (called on the island wild pepper), and near the observatory are evidently the remains of a quarry, whence the stones for its erection have been taken.

Halley does not appear to have selected the best site on the island as to meteorological conditions; at least, in the present day Ladder Hill is very much better.

To windward the island rises very abruptly nearly to its greatest height, and it is a very curious and interesting sight to watch the mountain ridge from a point near Diana's Peak, when the dew-point of the approaching trade current is a little below that of the temperature of the ridge.

Out to windward the sky is clear and blue, and so it remains over Sandy Bay (the low land between the ridge and the sea). Twenty or thirty yards below the crest of the ridge, and perhaps a hundred yards seawards, the cloud suddenly forms like a grey mist, rolls over the crest, and becomes thicker and blacker, depositing itself in rain as it passes over the high land, and then, as it descends to the lower land, it dissipates in the higher temperature, and over Ladder Hill all is again clear blue sky.

I found that Sabine's observations made at Longwood (nearly the most cloudy part of the island) did not at all represent the amount of cloud at Ladder Hill; and the reasons will be evident from the illustration I have given.

As night after night showed clear blue sky, and conditions generally favourable for astronomical purposes, I could not but regret that my pledges to the Society prevented my settling down here without incurring the further anxiety and risk of transhipping so much delicate apparatus. This regret was much increased by the great sympathy and assistance I should have received from the Governor. He is himself an enthusiastic amateur in astronomy, would have afforded every assistance in erecting the Observatory, and would himself have undertaken the time determinations.

On the afternoon of July 10 we left St. Helena, and on the morning of July 13 reached Ascension.* Two flags exhibited at the pier-head indicated that "double rollers" were in and that landing was dangerous; but we learned that preparations had been made to receive ourselves and baggage on board H.M. Supply Ship *The Industry*. Fortunately this became unnecessary, the rollers abated somewhat, we got safely ashore, our goods being placed in a large lighter, ready to be landed when the rollers would permit, and this was fortunately accomplished the

* It will be necessary to procure the Admiralty Chart of Ascension (surveyed by Lieut. Bedford, R.N.), 1838, to understand this Report.

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same evening. During the day I was busy inquiring as to a site. In answer to my questions about cloud, the reply of everyone was: "There is far too little of that; you will have no trouble from cloud," and so forth.

I knew that whatever cloud we might expect would probably be chiefly due to Green Mountain; it was generally cloud-capped, and so it was that day. I was told that often it was so, but that this cap of cloud simply remained there and did not affect the purity of the rest of the sky.

The house provided for our accommodation (photograph 1 enclosed) is situated in "Garrison" (George Town on the map), and "Garrison," excepting the Sanatorium "up the Green Mountain," is the only inhabited part of the island. To go "up the mountain" amongst the clouds was of course absurd; to get any point to windward* or south of Green Mountain I was told was impossible, because the lava was impassable by land, and there were no landing-places on the island where instruments could be put ashore excepting the pier-head and Comfortless Cove to the north of Garrison.

Garrison appeared to be, then, the only site, and exactly opposite our house—not twenty yards from our door—is what is called the "croquet ground," a large levelled space paved with concrete; and here I had all the baggage carted on the morning of July 14.

Notwithstanding a kind invitation from Captain Phillimore to spend a few days with him till we could get our house in order, we took up house at once, so as to be on the spot; and with the assistance of some blue-jackets and marine artificers, the Observatory (photograph 2) was erected in a couple of days.

At the end of five days all the instruments were established in perfect adjustment, with a distant meridian mark to the south, and a mark in focus of a long focus-lens as collimator to the north for the transit. The whole was as complete and comfortable as could be desired. For the first few nights after our arrival the sky was beautifully clear; but hardly were the instruments adjusted when cloudy weather set in and completely upset all my hopes of obtaining a series of observations of *Ariadne*.

After some vain attempts I was reluctantly compelled to give up the *Ariadne* investigation, and commenced the triangulation of the *Mars* stars of comparison, and for some days with considerable success.

Though the evenings and mornings were cloudy, still it was possible for some time to secure observations not far from the zenith, and in this way, by much labour and watching I was able to go over most of the principal triangulations once. Matters however got worse and worse, and I began to fear that some persistent cause was at work, and that the opinions I had gathered as to the weather were not trustworthy.

* Being in the trade-wind, this is S.E.

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Everyone declared that such a period of cloud had never been before observed; but it appeared to me that much of it was due to local causes. I observed frequently that whilst we had a band of cloud across the zenith, the horizon to north and south, particularly to south, was often clear.

The band of cloud undoubtedly came from Green Mountain, was carried over Garrison and out to sea; the point was, would it be possible to get south of this cloud, and, if possible, could an observatory be erected and maintained there?

Captain Phillimore, while warning me of the difficulties attending any change in the site of the Observatory, volunteered in the kindest manner to assist in every possible way, not only in any change I might desire to make, but also in the search of a site. The night of Monday, July 30, was typical of many that had gone before, and, that I might not lose any opportunity of observing that might occur, my wife volunteered to make the necessary observations. Accompanied by two servants, she set off at 10 P.M. and walked as far south as possible, making simultaneous observations with me at previously arranged instants. These observations were continued from 10 P.M. to 3 A.M. at each half-hour, and from these I was enabled to draw definite conclusions as to the cause and position of the cloud band.

These conclusions were briefly as follows:—

1. In certain conditions of the trade-wind the effect of the Green Mountain resembles the effect of introducing a lump of soft clay in a stream of clear water—a broad band of cloud is produced, which passes over the island in the direction of the wind. The cloud begins somewhat to windward of the peak.

2. The south limit of this stream of cloud passes nearly vertically over the south limit of "South-West Plain," about as far as "Round Hill." To clear this cloud it was evident that a point near the south limit of the island must be occupied, and also pretty far east, so that the cloud which may form windward of Green Mountain shall subtend the smallest possible angle.

The following day Captain Phillimore and I set out to explore. From South Gannet Hill we saw the small bay shown (unnamed) on the chart, due south of Saddle Crater, and one of the watercourses seemed to render it possible to reach this bay from the land side. It is impossible to describe the extraordinary character of this lava, but I hope to send some photographs of it by next mail; except in the beds of watercourses, it is quite impassable—the lava is so rugged and so sharp. Locally it is called "clinker." We reached the bay, and, after close examination, Captain Phillimore believed instruments could be landed on a smooth day. At the extreme north-east corner of the bay there is a little sandy beach, and the projecting rocks seemed entirely to protect it from the swell from the south-east. The official report in the hands of the Admiralty states that there is *no* landing place here, and the bay is not in sight of any signal station. Captain Phillimore was convinced of the justness of his conclusions, and undertook the

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responsibility of the landing, and when my friend Lieutenant Hammich, commanding H.M.S. *Cygnet*, undertook to give me Kroomen to carry the heliometer tube by land, I resolved to dismantle the Observatory the following day. That night (July 31) I obtained a fine set of evening and morning observations of *Mars* and comparison stars, and was strongly tempted by kind advisers to let well alone, and take my chance of some clear nights rather than incur the risk, anxiety, and labour of another transport and erection. I had, however, already weighed all these chances, and, accordingly, when the men arrived on Wednesday morning, we set to work, and by the same evening, with the aid of a considerable force of men, all the instruments and houses were dismounted, repacked, and stowed in a large lighter moored near the quay. At sunrise next morning (Thursday, August 2), the lighter and some small boats were towed round by a steam tug to our bay, now called Mars Bay. The day was unusually fine, and, under the skilful personal superintendence of Captain Phillimore, all the instruments, houses, &c. were safely landed without a scratch. Friday and Saturday were occupied in re-erecting the Observatories and instruments, and on Saturday evening the heliometer was adjusted, and, but for excessive personal fatigue, I might have obtained morning observations of *Mars*. On Sunday, August 5, I obtained good evening and morning observations of *Mars*.

During the day of the 5th August I found that, without some special arrangements, life would be unsupportable at Mars Bay, from flies, sandbugs, and other insects. I therefore returned by land (walking over the clinker) to Garrison, to procure carbolic acid, mosquito curtains, and other protections from insects, and returned the same night to make observations.

I had the misfortune to strain a tendon in my knee-joint (an old weakness), and reached Mars Bay, feeling very ill, and in considerable pain.

The night was fine, but when I attempted to observe I could not see, and was unable to stand. I sent for a doctor next day, who came and had me carried over the clinker by men, and conveyed to Garrison, where he found I was knocked up by exposure, anxiety, and fatigue, and was suffering from a local fever called the "Roller Fever." He promised I should be well in three days if I would place myself in his hands. Accordingly, on Friday the 10th, I was able to return to work, and secured morning observations of *Mars*. On Saturday the 11th we had rain, and after the early evening persistent cloud. I shall add at the end of this report what weather we have up till mail goes.

I can only now say that I have an Observatory here complete in all points. Complete arrangements have been made for the regular supply of food and water from Garrison, and such provision made for my own comfort as, I believe, will enable me to live without much loss of health. The best possible site in the island, in an astronomical point of view, has been secured, and we

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can now only wait for such weather as the trade-wind brings. The observations secured seem to be good; at least, the observer, in making them, has the satisfactory feeling that he is measuring a well-defined satisfactory object. The character of the definition is much more crisp and sharp, and the images are more steady here than in Garrison; and I look forward to a satisfactory determination of the parallax, if Providence sends me health and fine weather.

The following observations of *α Centauri* may be interesting to astronomers who are engaged on the orbit of that interesting binary.

α Centauri.

1877.	P. Angle. ° ′	Weight.	
July 22	69 25	½	Distance *about* 2″·0 or 2″·1.
27	80 35	2	I defer a more exact value till I have completed a reinvestigation of the screw.
28	75 17	1	
Aug. 5	80 33	2	

August 15.

Two additional morning observations of *Mars*, and one additional evening observation, have been secured: that is, in all, four evening and five morning observations. Much has been done within the past two days to render life comfortable; roads have been made, and the neighbouring patches of lava-dust covered with sand.

The following additional observation of *α Centauri* was made last night:—

1877.	P. Angle.	Weight.
Aug. 14	80° 42′	4

The weather shows signs of improvement.

The observations recently secured would have been lost in Garrison.

I trust the Society will find it possible to convey their thanks to Captain Phillimore for his great and successful exertions on behalf of the expedition.

The Observatory, Mars Bay,
Ascension, August 15, 1877.

Second Report, dated September 4, 1877.

By the mail which should leave September 5 (the day of *Mars'* opposition) I beg to forward the following report of the progress of the Society's Expedition.

In my last report I described the reasons which induced me

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to dismantle the Observatory in Garrison and re-erect it on its present site.

The necessity for this step has been abundantly proved by the result, and whereas on very few occasions would measures of any kind have been possible in Garrison, a considerable number have been secured here.

The amount of cloud has been unusually great, and this has made the labour of watching very excessive.

By dint of constant watching *Mars* has been observed since August 10 whenever visible between 20° and 75° zenith distance, except on two occasions, when the definition was unsatisfactory and it was not thought advisable to make observations.

Both these occasions were in the evening, and observations in satisfactory atmospheric conditions were secured at a later hour. The accompanying table will give a pretty accurate idea of the observations secured. Only those are entered which are satisfactory and complete, and capable of being used in a parallax determination.

The definition, with the exceptions above mentioned, has been exceedingly steady and sharp, and the conditions of temperature are even more satisfactory than I had anticipated.

The table attached gives the reading of the external thermometer before and after each set of evening and morning observations, and it will be seen on examination that not only is the temperature range between evening and morning confined to a little more than 1° F., but the extreme range throughout the whole series is less than 3° F. Though the observations are so arranged that a difference in temperature between the evening and morning observations can produce no systematic error in the final result, yet there is no doubt that this remarkable uniformity will tend to diminish the final probable error.

The Observatories and dwelling tents are now connected by good paths, and these and the patches of volcanic dust have been covered by rough-grained sand (particles of shell) from the beach. The supply of food and provisions has been systematically arranged. Thanks to the tents in store from the Ashantee Expedition, our tent accommodation is excellent, and generally, as to comfort and health, all is as well as need be. The ample time observations (generally made by day) are reduced, and their results, together with duplicates of the heliometer observations for preservation, will be forwarded by next mail. For the past few days there has been but little of the usual trade-wind, and the island has been covered by nearly continual cloud during the night; thus no observations have been possible during the important period since August 31, except during a break in the cloud for two hours on the evening of September 2. This change causes me much anxiety, occurring as it does at so critical a juncture. I earnestly trust the weather may prove favourable during the remaining half of the opposition, and that I may thus be able to report the entire success of the expedition.

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The accompanying photographs will illustrate the arrangement of the Observatory.

Mars Bay, Ascension Island,
September 4, 1877.

Third Report, dated October 5, 1877.

I am happy to report the success of the *Mars* observations, or at least their success so far as it is possible to judge without complete reduction.

Satisfactory observations have been secured on 32 evenings and 25 mornings, during which series the distance of the planet has been measured from 22 different stars of comparison, the whole embracing 327 complete measures of distance, of which 194 belong to the evening and 133 to the morning observations. A *complete measure* involves four bisections, eliminating index error and planet diameter, so that 1,408 bisections will be employed in the complete discussion of the observations. The rainy season, which I feared (as stated in my last report), actually set in, and entirely prevented observations from September 11 to September 15, and indeed more or less from September 11 to the end of the month, but most fortunately was preceded by exceptionally fine weather during the important period September 4 to September 11.

This latter period was of extreme importance, because—

1° September 5 was the day of opposition, and the planet also passed within 2′ of arc of the comparison star *g*. A very long series of this important appulse was obtained.

2° During the period September 5 to 11 the parallax was near its maximum, and also the planet was always nearly in the line joining the comparison stars *e* and *g*. This pair of stars has been used as the standard distance in the triangulation, and is therefore the best determined distance in the triangulation; the position angle of the line joining these stars differs only 5° from 90°, the direction of maximum diurnal displacement.

3° The position angle of greatest phase was during this period nearly at right angles to the direction *e–g*, and the maximum phase was very small; hence, along the line *e–g* the limbs were actually without phase.

For my own part I would willingly have undertaken the labour and anxiety of the expedition for the chances of this opportunity alone, and in its successful issue I hope the Society will not regret their liberality in promoting the expedition.

By this mail I send:

1. A copy of all the observations of *Mars*, with a short explanatory preface; this for preservation in the event of any accident to myself or to the original observations.

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2. An account of the time determinations and definitive table of their results.
3. An account of the chronometers and discussion of the definitive daily error of the observing chronometer, with table of this error for every day of the *Mars* observations.
4. Observations for errors of the micrometer screw.
5. A note on the geographical position of the Observatory at Mars Bay.

By next mail I hope to send a complete copy of the observations made in triangulating the stars of comparison, together with a chart showing the plan of the triangulation.

The following Observatories have promised co-operation in the work of making meridian observations of the comparison stars :—

Greenwich.

Paris.

Pulkowa.

Several German Observatories, through Dr. Auwers.

Cordoba (Dr. Gould).

Cambridge, Mass.

Ann Arbor, Michigan (also extra-meridian observations).

Mr. Hartnup has also promised extra-meridian observations at Liverpool.

I have also sent lists of the comparison stars and requests for co-operation to the following Observatories, from which no replies have as yet been received :—

Washington.

Cape of Good Hope.

*Melbourne.

Sydney.

Lord Lindsay has also promised observations at Dun Echt, so far as these will not interfere with the declination observations of *Mars*, and Dr. Ball at Dublin has also promised co-operation, conditionally on the state of health of his assistant Mr. Benton.

I hope that the result of these observations, combined with the heliometric triangulation, will give places of the comparison stars accurate to 1-10th of a second of arc at least, so that the differential equations resulting from the measures of distance may all be combined, neglecting the errors of the star places.

I regret that my last report (now enclosed) by an unfortu-

* *Sept.* 10, 1877.—Since the above Report was written, the mail has arrived, bringing a letter from Mr. Ellery, to say that the *Mars* comparison stars will be observed at the Melbourne Observatory.

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nate combination of circumstances missed the last mail, together with the rest of my month's correspondence. This accident is the more to be regretted, that some requests for special observations will, I fear, be too late now to be attended to so fully as I feel sure they would otherwise have been.

My health has somewhat suffered from the prolonged strain of the past two months, and I propose to-morrow to go "up the mountain" for a week's holiday and cool fresh air, and then to return to the completion of the triangulation of the *Mars* stars of comparison and the observations of *Melpomene*, when I hope the present period of cloudy weather will be past.*

Mars Bay, Ascension Island,
October 5, 1877.

On the Inferences for the Value of Mean Solar Parallax and other Elements deducible from the Telescopic Observations of the Transit of Venus, 1874, *December* 8, *which were made in the British Expedition for the Observation of that Transit.* By Sir G. B. Airy, K.C.B., Astronomer Royal.

The Society are aware that, in response to a question in the House of Commons, a Report of the results as to the value of the Mean Solar Parallax derived from the telescopic observations made in the British Expedition for observing the late Transit of *Venus* was prepared and printed in the past summer, and is now on sale with other parliamentary documents. Although all important points were fully treated in that Report, several minor matters were necessarily deferred; some which, in the first instance, did not present themselves for consideration; and some which, under pressure of time, it was difficult to examine. I propose, in the present Paper, to supplement the Parliamentary Paper; to describe the process there pursued up to the point of treatment of equations, omitting, however, the somewhat voluminous details; and to resume the equations and to treat them in what appears to be the legitimate form; presenting the results which, I think, necessarily follow from the British telescopic observations. At some future time, I trust, these observations, and these reductions (with any variations which time may suggest) in a more orderly form, and with the additional inferences to be derived from the photographic operations, will be printed under the auspices of the British Government.

Acting as official superintendent, and under official responsibility, I have examined and approved every part of the operation. But I am bound to state that the most important discrimination of classes of observations, the scrutiny of individual observations, the organisation of an efficient computing staff, and

* The statements of the observations of *Mars* which form part of these Reports are printed p. 17 *et seq.*—Ed.

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the general maintenance of order in the calculations, are due to Captain G. L. Tupman, R.M.A., head of the British Expedition for observing the Transit of *Venus*.

The printed paper to which I have alluded specifies the elements of geographical position and the evidence for accuracy of longitude at each of the stations. It contains also the description of the classes of observations to which the observers had attached definite times. At ingress it was usually possible to distinguish three classes of recorded observation, which, for convenience, were marked α, β, γ, of which β appeared to be most certain. At egress the classes (in the opposite order as regarded change of angular interval between the limbs) were δ, ϵ, ζ, η; ϵ or ζ appeared to correspond with β (I think ϵ the better, but ζ was more frequently noted), and η corresponded vaguely with α. The observations of all these classes are carefully distinguished and fully exhibited.

The order of reductions then adopted was as follows:—The observations were first divided into the two great groups of Ingress and Egress, which were kept separate to the end. Then the group of Ingress was divided into different pairs of comparison (accelerated by parallax at Sandwich Islands with retarded at Kerguelen; accelerated at Sandwich Islands with retarded at New Zealand; retarded at New Zealand with retarded at Rodriguez; retarded at New Zealand with retarded at Kerguelen). And the group of Egress was similarly divided into pairs (retarded at Mokattam and Suez with retarded at Rodriguez; retarded at Mokattam and Suez with accelerated at Kerguelen; retarded at Thebes (Luxor) with retarded at Rodriguez; retarded at Thebes with accelerated at Kerguelen). And in each of the pairs for Ingress, the α, the β, the γ, of one station was compared separately with the α, the β, the γ, of the other; and in each pair for Egress, the δ, the ϵ, the ζ, of one station was compared separately with the δ, the ϵ, the ζ, of the other.

The reduction of a comparison of observations made at two stations was thus completed. Adhering to the first steps in my Paper printed in the *Monthly Notice* of 1874, December 8, the registered times of all observations were converted into Greenwich Sidereal Times; and with these, and with a table of Tabular Places and Parallaxes which has long since been circulated, the Tabular Geocentric Places and Tabular Local Parallaxes were prepared, and the apparent Local Tabular Distance between the centres of the Sun and *Venus*, affected in each case by a symbolical multiple n of the effect of increasing Tabular Parallax by $\frac{1}{100}$ part, was computed for every observation. This apparent local distance between centres, thus computed for Greenwich time of observed contact of limbs, ought to agree with the simple tabular assumed difference of semidiameters; and, for each

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station, an equation was formed, asserting this equality, affected only by errors in the places and in the semidiameters of the Sun and planet. The effects of these latter errors were assumed to be equal at the observations made at the two stations, and therefore disappeared in the comparison of the two equations, leaving as an unknown term the multiple of n. The numerical value of n being found from this equation, and the assumed numerical value of the Sun's mean parallax adopted in the calculation being $8''{\cdot}950$, the true value as found from these observations is $8''{\cdot}950 \times \left(1 + \frac{n}{100}\right)$.

After carefully considering all the results for n, and combining them with proper weights, the values obtained for Mean Solar Parallax were the following:

By Ingress, $8''{\cdot}739$, weight 10·46;
By Egress, $8''{\cdot}847$, weight 2·53;
General Result, $8''{\cdot}760$.

In order to insure general correctness of the result, and also to ascertain the values of the errors in the tabular places of the Sun and *Venus*, and to show how all may be affected by possible errors of observation &c., I have now thought it best to form equations strictly in the manner described in my paper of 1874, December 8, and to treat the whole mass of equations, both for Ingress and for Egress, in one united assemblage, by the method proper for combination of equations.

To prepare for this operation it appeared desirable, if possible, to reduce the times of all the observations at Ingress to the times which would have been adopted by the observers for phase β, and to reduce all at Egress to those for phase ζ. For this purpose comparisons were made of all observations which could connect the different phenomena. The results obtained were

The observation α is *earlier* than β by $24^s{\cdot}22$; { and the angular distance between centres of Sun and planet is *greater* at α than at β } by $0''{\cdot}78$.

The observation γ is *later* than β by $20^s{\cdot}17$; { and the angular distance is *less* at γ than at β } by $0''{\cdot}64$.

The observation δ is *earlier* than ζ by $48^s{\cdot}4$; { and the angular distance is *less* at δ than at ζ } by $1''{\cdot}63$.

The observation ϵ is *earlier* than ζ by $21^s{\cdot}0$; { and the angular distance is *less* at ϵ than at ζ } by $0''{\cdot}72$.

The observation η is *later* than ζ by $72^s{\cdot}1$; { and the angular distance is *greater* at η than at ζ } by $2''{\cdot}41$.

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No use is made of the observations η.

The tabular apparent distances between the limbs of the Sun and *Venus*, having been compnted for the registered time of every observation, were then affected with these corrections, and were considered to represent the true tabular distances of limbs, subject to the assumption that the phase β for Ingress and the phase ζ for Egress, intended to correspond to apparent contact of the true limbs of the Sun and *Venus*, may require corrections which I shall call β and ζ; that the tabular semidiameters of the Sun and planet require the corrections δR and δr, and their difference therefore requires the correction $\delta R - \delta r$, which I shall call ρ; and that the excesses of the Right Ascension and North Polar Distance of *Venus* above those for the Sun require the corrections δ.RA and δ.NPD. There may be a clock-error or error of longitude affecting all the times of observation at one station, and therefore all the distances of limbs at one station, by the same quantity, different for different stations: for these the symbols t_1, t_2, &c. are used. When all these corrections are applied, the tabular distances of limbs ought to $= 0$. This is the requirement which defines the form to be used for the equations; it is assumed that the probable error in satisfying this requirement is the same for each equation.

The equation was formed for every recorded observation. I do not here give all the details; I may, however, state that the number of observations was 54, made by 18 observers; and the number of equations was therefore 54.

The following are the sums of the equations at each station. It is to be remarked that δ.RA, δ.NPD, and $\rho - \beta$, are expressed in seconds of arc; that t_1, t_2, &c. are expressed in seconds of time; and that the other coefficients are abstract numbers.

Ingress at Honolulu, 10 observations:

$$+78''{\cdot}40 = -2''{\cdot}280 \times n + 6{\cdot}5324 \times \delta.\mathrm{RA} - 7{\cdot}0606 \times \delta.\mathrm{NPD} - 10 \times (\rho - \beta) - 0''{\cdot}342 \times t_1.$$

Ingress at Rodriguez, 8 observations:

$$+54''{\cdot}14 = +1''{\cdot}554 \times n + 4{\cdot}8868 \times \delta.\mathrm{RA} - 5{\cdot}9946 \times \delta.\mathrm{NPD} - 8 \times (\rho - \beta) - 0''{\cdot}247 \times t_2.$$

Ingress at Kerguelen, 9 observations:

$$+61''{\cdot}03 = +1''{\cdot}937 \times n + 5{\cdot}3908 \times \delta.\mathrm{RA} - 6{\cdot}8446 \times \delta.\mathrm{NPD} - 9 \times (\rho - \beta) - 0''{\cdot}267 \times t_3.$$

Ingress at New Zealand, 2 observations:

$$+14''{\cdot}20 = +0''{\cdot}028 \times n + 1{\cdot}2347 \times \delta.\mathrm{RA} - 1{\cdot}4861 \times \delta.\mathrm{NPD} - 2 \times (\rho - \beta) - 0''{\cdot}064 \times t_4.$$

Egress at Mokattam and Suez, 7 observations:

$$+26''{\cdot}01 = -1''{\cdot}415 \times n - 1{\cdot}7146 \times \delta.\mathrm{RA} - 6{\cdot}7488 \times \delta.\mathrm{NPD} - 7 \times (\rho - \zeta) + 0''{\cdot}239 \times t_5.$$

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Egress at Luxor, 5 observations:

$$+19''\cdot17 = -0''\cdot954 \times n - 1\cdot2022 \times \delta.\mathrm{RA} - 4\cdot8272 \times \delta.\mathrm{NPD} - 5 \times (\rho - \zeta) + 0''\cdot169 \times t_6.$$

Egress at Rodriguez, 9 observations:

$$+34''\cdot73 = -0''\cdot245 \times n - 1\cdot9692 \times \delta.\mathrm{RA} - 8\cdot7432 \times \delta.\mathrm{NPD} - 9 \times (\rho - \zeta) + 0''\cdot294 \times t_7.$$

Egress at Kerguelen, 4 observations:

$$+14''\cdot09 = +0''\cdot403 \times n - 0\cdot7834 \times \delta.\mathrm{RA} - 3\cdot9091 \times \delta.\mathrm{NPD} - 4 \times (\rho - \zeta) + 0''\cdot125 \times t_8.$$

It was then necessary to consider the details in the mode of combining these equations, and the subjects to which the solution should be directed.

The equations are favourable for the determination of n, of $\delta.\mathrm{RA}$, and of $\delta.\mathrm{NPD}$. But when we examine the position of $\rho - \beta$ and $\rho - \zeta$, a new consideration presents itself. The quantity ρ as a matter affecting observation is not independent of $\delta.\mathrm{RA}$ and $\delta.\mathrm{NPD}$. We can, by alteration of $\delta.\mathrm{RA}$ and $\delta.\mathrm{NPD}$, alter ρ by any amount, and by the same amount at Ingress and at Egress. I have thought it best, therefore, to place ρ, as an undetermined constant, on the constant side of the equation, and, carrying its factor through all the operations which lead to ultimate solution, to exhibit its effect on the final results for n, for $\delta.\mathrm{RA}$, and for $\delta.\mathrm{NPD}$. And I have treated the effects of possible errors of Greenwich time on each sum of equations (t_1, t_2, &c. multiplied by the number of equations) in the same way.

For correct application of the Theory of Probabilities, it is necessary to give such a form to each of the equations which are to be collected that the probable errors of all will be equal. Let e be the probable error of the equation given by one observation (all being supposed liable to the same chance-error, as has been mentioned). Then the probable error of the sum of the ten equations at Honolulu will be $e\sqrt{10}$; the sum of the ten equations must therefore be divided by $\sqrt{10}$, and thus an equation is formed whose probable error is e. Now to form the Normal Equation most favourable for the determination of n (for instance), this equation of Honolulu (already formed by dividing the sum of equations by $\sqrt{10}$) must be multiplied by its coefficient of n (which has been formed by dividing the coefficient of n, in the sum of equations, by $\sqrt{10}$). And therefore, if the sum of equations for Honolulu be multiplied by its coefficient of n, the product must be divided by 10 in order to form the equation proper to enter into the combination which forms the Normal Equation. The same applies to $\delta.\mathrm{RA}$, $\delta.\mathrm{NPD}$. The general error of time t_1 will be simply multiplied by the reduced coefficient of n.

Similar remarks apply to the other stations.

The Normal Equations finally formed are the following:—

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$$(1) \quad -2{\cdot}4658 = \left\{ \begin{array}{l} +1{\cdot}7543 \times n + 1{\cdot}1881 \times \delta.\mathrm{RA} + 1{\cdot}0807 \times \delta.\mathrm{NPD} - 1{\cdot}2390 \times (\rho-\beta) + 2{\cdot}2110 \times (\rho-\zeta) \\ +{\cdot}0780 \times t_1 - {\cdot}0480 \times t_2 - {\cdot}0575 \times t_3 - {\cdot}0009 \times t_4 - {\cdot}0483 \times t_5 - {\cdot}0322 \times t_6 - {\cdot}0080 \times t_7 + {\cdot}0126 \times t_8 \end{array} \right\}$$

$$(2) \quad +108{\cdot}2705 = \left\{ \begin{array}{l} +1{\cdot}1881 \times n + 12{\cdot}5369 \times \delta.\mathrm{RA} - 7{\cdot}8001 \times \delta.\mathrm{NPD} - 18{\cdot}0450 \times (\rho-\beta) + 5{\cdot}6695 \times (\rho-\zeta) \\ -{\cdot}2234 \times t_1 - {\cdot}1509 \times t_2 - {\cdot}1599 \times t_3 - {\cdot}0395 \times t_4 - {\cdot}0585 \times t_5 - {\cdot}0406 \times t_6 - {\cdot}0643 \times t_7 - {\cdot}0245 \times t_8 \end{array} \right\}$$

$$(3) \quad -243{\cdot}9714 = \left\{ \begin{array}{l} +1{\cdot}0807 \times n - 7{\cdot}8001 \times \delta.\mathrm{RA} + 39{\cdot}2672 \times \delta.\mathrm{NPD} + 21{\cdot}3858 \times (\rho-\beta) + 24{\cdot}2283 \times (\rho-\zeta) \\ +{\cdot}2415 \times t_1 + {\cdot}1851 \times t_2 + {\cdot}2031 \times t_3 + {\cdot}0476 \times t_4 - {\cdot}2304 \times t_5 - {\cdot}1632 \times t_6 - {\cdot}2856 \times t_7 - {\cdot}1222 \times t_8 \end{array} \right\}.$$

And their solutions are:—

$$n = \left\{ \begin{array}{l} -2{\cdot}1911 + 0{\cdot}0429 \times (\rho-\beta) - 0{\cdot}1331 \times (\rho-\zeta) \\ -{\cdot}0606 \times t_1 + {\cdot}0249 \times t_2 + {\cdot}0310 \times t_3 - {\cdot}0011 \times t_4 + {\cdot}0185 \times t_5 + {\cdot}0118 \times t_6 - {\cdot}0100 \times t_7 - {\cdot}0143 \times t_8 \end{array} \right\}$$

$$\delta.\mathrm{RA} = \left\{ \begin{array}{l} +5''{\cdot}7227 - 1{\cdot}2502 \times (\rho-\beta) - 0{\cdot}9371 \times (\rho-\zeta) \\ +''{\cdot}0237 \times t_1 + ''{\cdot}0072 \times t_2 + ''{\cdot}0069 \times t_3 + ''{\cdot}0029 \times t_4 + ''{\cdot}0071 \times t_5 + ''{\cdot}0050 \times t_6 + ''{\cdot}0123 \times t_7 + ''{\cdot}0062 \times t_8 \end{array} \right\}$$

$$\delta.\mathrm{NPD} = \left\{ \begin{array}{l} -5''{\cdot}0164 - 0{\cdot}2977 \times (\rho-\beta) - 0{\cdot}7995 \times (\rho-\zeta) \\ +''{\cdot}0001 \times t_1 - ''{\cdot}0039 \times t_2 - ''{\cdot}0046 \times t_3 - ''{\cdot}0005 + t_4 + ''{\cdot}0069 \times t_5 + ''{\cdot}0051 \times t_6 + ''{\cdot}0101 \times t_7 + ''{\cdot}0046 \times t_8 \end{array} \right\}$$

where $\rho-\beta$ and $\rho-\zeta$ are expressed in seconds of arc, and t_1, t_2, &c. in seconds of time.

The value of Equatoreal Mean Solar Parallax, corresponding to $n = -2{\cdot}1911$, is $8''{\cdot}754$; and the mean distance of the Sun's centre is 93,375,000 miles.

In adopting for the results the form in which they are here exhibited, I have endeavoured to make them the most complete which it is possible to extract from the Telescopic Observations of the Transit of *Venus* now treated.

Royal Observatory, Greenwich, 1877, *November* 6.

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Statement of Mr. Gill's Observations of Mars obtained at Ascension between July 31 and September 4, both inclusive.

	Date.	Evening Observations. Stars of Comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	Morning Observations. Stars of Comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	
	1877.		h m	°	in.		h m	°	in.	
Shifting from Garrison to Mars Bay.	July 31	$q_2\ n_2\ t_2$	4 30	74·5	30·167	$q_2\ n_2\ t_2$	0 27	72·6	30·164	Made at Garrison.
			2 32	73·5	30·150		2 40	72·8	30·116	
	Aug. 5	$q_2\ n_2\ t_2$	4 51	75·0	30·223	$q_1\ t_1\ n_2$	1 7	73·5	30·164	Made at Mars Bay
Ill in bed			3 21	74·0	30·223		2 51	74·2	30·183	
	Aug. 10	...	...	...	...	$q_1\ t_2\ n_2$	2 43	73·6	30·190	do.
							3 45	73·5	30·209	
	Aug. 12	$q_2\ n_2\ t_2$	4 35	73·0	30·201	...	...	...	...	do.
			3 21	72·7	30·195					
	Aug. 13	...	...	...	...	$q_1\ t_2\ n_2$	2 3	71·7	30·164	do.
							2 59	71·9	30·175	
	Aug. 14	$q_2\ n_2\ t_2$	5 18	73·0	30·188	$q_1\ t_1\ n_1$	1 59	72·5	30·162	do.
			3 23	72·7	30·199		3 11	72·1	30·176	

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Date.	Evening Observations. Stars of comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	Morning Observations. Stars of comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	
1877.		h m	°	in.		h m	°	in.	
Aug. 15	$q_2\ s_2\ n_2$	4 2	73·8	30·177	$q_1\ s_1\ n_2$	1 55	72·2	30·157	Made at Mars Bay.
		2 58	73·0	30·150		3 29	73·0	30·130	
Aug. 16	$q_2\ s_2\ n_2$	4 17	73·5	30·173	$q_1\ s_1\ n_1$	3 34	73·2	30·134	do.
		2 50	73·3	30·179		4 2	72·3	30·146	
Aug. 19	$m_2\ n_2\ r_2$	4 34	73·5	30·183	$m_1\ n_1\ r_1$	2 49	73·3	30·143	do.
		3 27	73·1	30·209		3 29	73·1	30·193	
Aug. 20	$n_2\ r_2\ \lambda_2$	4 14	74·0	30·184	$n_1\ r_2\ \lambda_2$	1 51	72·3	30·180	do.
		3 11	73·3	30·192		3 31	72·8	30·138	
Aug. 21	...	...	...	...	$n_1\ r_2\ \lambda_2$	1 40	72·7	30·117	do.
						4 37	72·0	30·130	Much cloud; long delay.
Aug. 23	$n_1\ r_3\ l_2$	4 51	74·3	30·164	...	...	...	...	Made at Mars Bay.
		2 33	73·7	30·182					
Aug. 24	$n_2\ q_3\ l_2$	4 43	74·7	30·150	$q_3\ l_3\ n_2$	1 58	73·1	30·122	do.
		2 32	74·0	30·175		4 4	74·4	30·119	

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Aug. 26	$n_2\ q_3\ l_2$	4 44	74·5	30·100	...	...	...	...	do.
		2 24	73·8	30·123					
Aug. 27	$n_2\ h_2\ l_2\ m_2\ \lambda_1$	4 56	73·9	30·113	...	...	...	...	do.
		2 15	73·5	30·137					
Aug. 29	$i_1\ m_2 f_1\ h_1\ \lambda_1$	4 35	73·5	30·150	$\lambda_1\ h_2\ m_2 f_1\ i_1$	1 37	72·1	30·096	do.
		2 53	73·0	30·150		3 31	72·9	30·082	
									do.
Aug. 31	$f_2\ m_2$	3 36	74·2	30·142	$f_2\ m_2$	1 30	72·9	30·100	The planet in the line joining the stars; third star not required.
		2 29	73·7	30·153		3 0	73·0	30·100	
Sept. 2	$g_3\ \lambda_2\ k_1\ i_1$	3 43	74·2	30·116	...	...	...	...	Made at Mars Bay.
		1 51	73·2	30·150					
Sept. 4	$f_2\ k_2\ g_4$	4 48	73·9	30·122	...	...	...	...	do.
		3 4	73·3	30·150					

The figures subfixed to the letters indicating the stars of comparison observed show how many *complete measures* of the distance of the planet from the star have been observed.

A *complete measure* means four bisections so arranged as to eliminate index error of the instrument and error in the assumed diameter of the planet.

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Statement of Mr. Gill's Observations of Mars obtained at Ascension between September 5d·0 and the end of the Series of Observations.

Date. 1877.	Evening Observations. Stars of comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	Morning Observations. Stars of comparison observed.	Hour-angle of first and last Obs.	Temp. (F.) before and after Obs.	Barom. before and after Obs.	
		h m	°	in.		h m	°	in.	
Sept. 5	$g_6\ f_1\ h_1$	4 39	74·2	30·169	g_{12}	0 37	72·7	30·168	
		2 58	73·5	30·189		4 15	71·9	30·128	
Sept. 6	$f_8\ g_4\ e_2\ h_1$	4 47	74·1	30·170	$g_2\ e_2\ h_1$	1 28	72·4	30·164	
		2 21	73·0	30·191		3 38	72·0	30·139	
Sept. 7	$g_1\ e_2\ h_1\ d_1\ f_1\ k_1\ \gamma_1$	4 37	74·3	30·166	$e_1\ g_2$	1 36	72·6	...	Much cloud & delay
		2 28	73·0	30·200		3 51	72·7		
Sept. 8	$g_2\ e_2\ h_2$	4 32	74·1	30·174	$d_1\ k_1\ \gamma_1\ f_1\ g_1\ e_2\ h_2$	1 3	72·5	30·195	
		2 38	73·4	30·195		4 20	72·6	30·164	
Sept. 9	$g_1\ e_1$	3 27	73·6	30·180	$d_1\ \theta_1\ g_4\ e_4$	1 36	73·0	30·154	
		3 4	73·7	30·196		4 31	72·7	30·142	
Sept. 10	$g_2\ e_2\ \theta_1\ d_1\ f_1\ \gamma_1$	3 37	74·0	30·162	$g_4\ e_4$	2 31	72·5	30·150	
		1 44	73·2	30·186		4 16	72·0	30·137	
Sept. 11	$g_3\ e_2\ \mu_2$	4 36	74·2	30·162					
		3 10	73·4	30·199					

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Sept. 15	c_1 e_1	3 32	73·0	30·132	...	...	...	...	The distance μ is very small. It is proposed to combine these, assuming a uniform scale value for both nights.
		3 9	not read						
Sept. 16	μ_2	4 0	72·5	30·088	...	...	...	...	
		3 40	not recorded						
Sept. 17	c_2 e_2 β_2 μ_2	4 22	72·3	30·064	β_1 e_1	3 2	71·5	30·091	Long interruption by cloud.
		1 28	71·8	30·100		4 40	69·7	30·050	
Sept. 19	e_2 b_2 c_1	3 41	72·9	30·070	e_2 b_2 c_1	1 20	72·1	30·100	
		2 14	72·0	30·096		4 0	71·5	30·036	
Sept. 22	μ_1 c_1	1 47	....	...	c_1	1 45	72·3	30·092	
		1 27	73·3	30·100		1 59	73·0	30·080	Read after long delay.
Sept. 23	μ_2 b_2 c_1	4 24	74·0	30·066					
		3 7	73·0	30·093					
Sept. 24	a_1 b_2 c_1 β_2	4 9	73·3	30·055	a_1 b_1 c_1 β_2	2 19	72·2	30·083	
		2 9	72·7	30·100		3 50	70·8	30·045	
Sept. 25	...	...	...	...	a_1 b_1 c_1 β_1	2 44	73·0	30·084	
						4 35	72·3	30·058	
Sept. 27	a_2 b_1 c_1 β_1	3 34	74·0	30·134					
		2 32	73·2	30·144					
Oct. 3	a_2 b_2	3 44	...	...	a_2 b_2	1 31	73·0	30·136	
		1 41	74·3	30·136		4 28	72·0	30·094	

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On the Proper Motions of Groombridge 2275 *and* 2276 (Σ. 1972).

By E. Dunkin, Esq. F.R.S.

The components of this interesting pair of stars, first observed by W. Herschel on 1782, October 12, and afterwards by J. Herschel and South, and W. Struve, under the designation of π' *Ursæ Minoris*, are separated by more than 30″, and according to Struve they are of the sixth and seventh magnitude respectively. There has been some evidence to support the notion that they are physically associated, but little can be gathered in confirmation from the measures hitherto made; on the contrary, the great angular distance between them would rather lead one to suppose that they are seen only optically double. There are, however, several pairs, or groups, of stars forming stellar systems much more widely separated, including some of those recently pointed out by M. Flammarion. Mr. Carrington must have believed in the affinity of these stars, for in his Redhill Catalogue he has inserted a note opposite Groombridge 2276, stating that its place is affected by "proper motion and rotation," to account for an apparent discordance between his resulting R.A. and N.P.D. and that derived from the observations of Professor Schwerd, of Speyer, without, however, giving any authority for the remark. The exact value of the annual proper motions was probably not known to Mr. Carrington, though he had ample data at his command for a fair approximation. It is now found that they are of nearly equal amount— −0ˢ·067 and −0ˢ·056 respectively in R.A., and −0″·13 and −0″·17 in N.P.D.; but, considering that the stars are situated only about nine degrees from the Pole, these quantities are not very remarkable. They are, however, of sufficient magnitude to justify their record in the *Monthly Notices*.

I have lately had occasion to verify the mean R.A. and N.P.D. of these stars given in the Greenwich annual star Catalogue for 1875, January 1, by bringing up to the same epoch the R.A. and N.P.D. inserted in the Groombridge and Radcliffe Catalogues. In doing this, my attention was drawn to the large and nearly equal discordances between the computed and the Greenwich Catalogue places, caused evidently by the separate undetermined proper motions of the stars. The discordances both in R.A. and N.P.D. were considerable, amounting to several seconds in each. Both stars were fortunately observed by Lalande on 1790, March 21, and the reduced mean places for January 1 of that year are contained in Fedorenko's Catalogue of his circumpolar stars. The long interval between 1790 and the dates of the modern observations is especially favourable for an accurate determination of the annual proper motion of each star in both elements. It has been considered sufficient for the purpose of our inquiry to confine the comparison of Lalande's places

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with those in the Radcliffe, Redhill, and Greenwich (1875) Catalogues. A separate comparison between the Groombridge and Greenwich places has, however, also been made as a check on the accuracy of the adopted values.

Although it seems premature at present to say that this pair of stars belongs to one stellar system, for the evidence is still insufficient, yet their resulting values of proper motion show conclusively that the two stars are travelling in space in the same direction with an almost equal velocity. The small change in their angular position and distances since the first observations of W. Herschel proves that they are too far separated to have much apparent gravitational effect on each other's movements, but, on the other hand, they appear to be sufficiently near to be mutually influenced by one common law of motion. The discordances between the different early measures of angular distance are greater than one would have expected, or we might probably have been able to give a more definite opinion. More light would, however, be thrown on the separate movements of these two stars if an accurate series of micrometrical measures of their angular position and distance were made at the present time to compare with the former measures. A few more meridional observations of the R.A. and N.P.D. of both stars are also desirable.

The following tabular statement contains the excess of the mean R.A. and N.P.D.—derived from Fedorenko's Lalande by the application of the geometrical precession—over the respective R.A. and N.P.D. printed in the Radcliffe and Redhill Catalogues, and in the annual star catalogue in the *Greenwich Observations* for 1875 :—

Catalogue.	Interval between Mean Epochs of Observation.	Excess of R.A. in Catalogues over R.A. from Lalande.		Annual Proper Motion in R.A.	
		G. 2275.	G. 2276.	G. 2275.	G. 2276.
	Years.	s	s	s	s
Radcliffe	60·0	−3·87	−3·29	−0·0645	−0·0548
Redhill	66·2	−4·35	−3·72	−0·0657	−0·0562
Greenwich	85·2	−6·00	−4·73	−0·0704	−0·0555

Catalogue.	Interval between Mean Epochs of Observation.	Excess of N.P.D. in Catalogues over N.P.D. from Lalande.		Annual Proper Motion in N.P.D.	
		G. 2275.	G. 2276.	G. 2275.	G. 2276.
	Years.	"	"	"	"
Radcliffe	55·5	−6·33	− 8·41	−0·114	−0·152
Redhill	66·2	−9·69	−13·11	−0·146	−0·198
Greenwich	85·2	−9·95	−13·26	−0·117	−0·156

The means of the annual proper motions resulting from the three comparisons are as follow :—

	Annual Proper Motion In R.A.	In N.P.D.
	s	"
Groombridge 2275	−0·0669	−0·126
„ 2276	−0·0555	−0·169

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These values have been used whenever required; but, as a check on the accuracy of Lalande's places, I have thought it desirable to make an additional comparison by substituting the Groombridge Catalogue for Fedorenko's Lalande. The Groombridge N.P.D. of 2276 is apparently more than four seconds in error, as may be observed below; the proper motion in N.P.D. of this star deduced from this second comparison is therefore too small. The remaining values are almost identical with those determined from the comparison with Lalande. The following are the resulting annual proper motions, using the Groombridge and Greenwich (1875) places only:—

	P.M. in R.A. s	P.M. in N.P.D. ″
Groombridge 2275	−0·064	−0·12
„ 2276	−0·055	−0·10

The following differences between the R.A. and N.P.D. of the two stars are taken from the mean places given in the respective catalogues:—

Catalogue.		Δ R.A. s	Δ N.P.D. ″
Fedorenko's Lalande	(1790)	12·50	0·0
Groombridge	(1810)	12·66	−5·1
Schwerd	(1828)	12·80	−0·4
Struve	(1830)	12·70	−0·8
Radcliffe	(1845)	12·80	−2·9
Redhill	(1855)	12·80	−4·4
Greenwich	(1875)	12·85	−4·6

Kidbrooke, Blackheath,
1877, *October* 22.

Note on the supposed Physical Association of Groombridge 3477 *and* 3511. By E. Dunkin, Esq. F.R.S.

In M. Flammarion's paper, "Sur un système stellaire en movement propre rapide" (*Comptes Rendus*, No. 8, August 20, 1877), he has exhibited what seems to be a remarkable instance of two stars, separated by more than six minutes of right ascension and nearly ten minutes of polar distance, travelling apparently together in space in the same direction with almost equal proper motions. Assuming that the proper motions employed by him are correct, he concludes that the association of these stars, one of which is a close double, forms a *stellar system* resembling in a great measure the stars 36 *Ophiuchi* and 30 *Scorpii*, which are separated as much as 12′ 21″ in a great circle, but have a common proper motion of some magnitude.

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Perhaps I may be permitted to remark that a few years ago the large proper motion in R.A. of one of the stars in question, Groombridge 3511, inserted in the British Association Catalogue, appeared to me to be much too great, and consequently a new value was determined by comparing the Groombridge place with that observed in recent years at Greenwich. This new value has been referred to in a footnote on page 16 of the annual star Catalogue in the volume of *Greenwich Observations* for 1871 as follows:—"The proper motion, $+0^s.049$, adopted for this star is determined from a comparison of the R.A. in Groombridge's Catalogue with the R.A. from Greenwich observations made between 1871, October 17, and 1873, March 27. The value, $+0^s.119$, inserted in the British Association Catalogue, appears to be erroneous." A more recent comparison of the Groombridge and Radcliffe places with the observations made at Greenwich from 1871 to 1875 gives respectively for the annual proper motion in R.A. $+0^s.048$ and $+0^s.053$. To these I add another determination from recent Greenwich observations only, from four made in 1860 and 1861, and seven made since the beginning of 1871. The result is remarkably accordant with the others, though the interval is only twelve years.

	Annual Proper Motion in R.A.	Annual Proper Motion in N.P.D.
	s	"
Groombridge with Greenwich (1872)	+0.048	−0.01
Radcliffe " "	+0.053	+0.07
Greenwich (1860–61) "	+0.049	+0.05

The means of these values are respectively $+0^s.050$ in R.A. and $+0''.043$ in N.P.D.

The evidently erroneous values, $+0^s.119$ and $-0''.10$, given in the British Association Catalogue having been employed by M. Flammarion in his investigation, his conclusions respecting the probable physical association of these two Groombridge stars will require some reconsideration.

Kidbrooke, Blackheath,
1877, *October* 30.

Note upon the Proper Motion of Groombridge 3511.

By Prof. T. H. Safford.

The supposed proper motion of the star Groombridge 3511

$+0^s.119$ and $+0''.10$

in Right Ascension and Declination respectively, as given in the British Association Catalogue, is derived from the comparison of

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Pond's position for 1830 with that of Groombridge 1810. It is, however, too large, and the component in Declination has the wrong sign, as will appear from the following tabulation of the positions for 1855, as brought up in Argelander's manner, by the application, namely, of the systematic corrections given in volume vii. (Introduction) of the Bonn observations, and of precession, secular variation, and the third term of the series as computed by means of Bessel's constants of precession, and an approximate position for 1855.

The precession formulæ from 1855 to $1855+t$ are for this star,

$$\alpha'-\alpha = -1^s\cdot52163\,t-0^s\cdot2656\,\frac{t^2}{200}-0^s\cdot537\left(\frac{t}{100}\right)^3,$$

$$\delta'-\delta = +15''\cdot8540\,t-0''\cdot1435\,\frac{t^2}{200}-0''\cdot427\left(\frac{t}{100}\right)^3.$$

In the following table the later observations receive only approximate systematic corrections, and in Declination zero; as Argelander found large discrepancies near the Pole, owing to the few stars of the *Tabulæ Reductionum* in that region. The observations denoted by H. C. were made at Cambridge, partly by myself, and reduced by the help of the *Berliner Jahrbuch*.

I have here at my command an old transit instrument by Simms, and will observe the star again so soon as the instrument shall be repaired. The error in the B.A.C. proper motion, like that of many other stars, was noted by me long ago; and I have in hand a series of several hundred stars the proper motion whereof has been incompletely discussed elsewhere, of which this is one.

The columns C−O contain the differences in hundredths of seconds of time and in seconds of space respectively from the formulæ

$$\alpha = 21^h\,28^m\,57^s\cdot09+0^s\cdot016\,(t-1855),$$
$$\delta = 79^\circ\,53'\,29''\cdot2\,-0''\cdot018\,(t-1855).$$

The resulting annual proper motion, $+0^s\cdot016$ (in R.A.) and $-0''\cdot018$ in Declination, is still a little uncertain in amount.

The collection of observations may be very easily incomplete, as I have not at hand all the Catalogues which probably contain the star.

Williams College, Williamstown, Mass., U.S.A.,
September 21, 1877.

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Catalogue.	Epoch. Obs.	Epoch. Pos.	R.A. at Epoch. h m s	Corr. s	Decl. at Epoch. ° ′ ″	Corr. ″	Position, 1855. h m s	Position, 1855. ° ′ ″	No. of Obs.	C−O R.A.	C−O Decl.
F	1790·1	1790	21 30 30·67	+0·026	79 36 17·78	+0·17	21 28 57·25	79 53 31·2	4	−120	−0 8
L.L.	1790·4	1800	30 15·35	+0·768	38 59·95	+0·50	56·36	33·5	2	− 31	−3·1
Gr.	1809·7	1810	30 0·18	+0·726	41 32·4	+1·31	55·07	28·6	6	+130	+1·4
Pd.	1830·9	1830	...	...	46 51·77	+0·49	...	29·1	8	...	+0·5
	1831·7	1831	29 32·07	−0·037	47 8·55	+0·49	56·27	29·9	24	+ 45	−0·3
	1832·8	1832	29 31·01	−0·037	...	...	56·67	...	5	+ 6	...
Pulk.	1842·2	1845	29 12·13	+0·038	...	...	57·08	...	2	− 19	...
R.C.	1850·7	1845	...	...	50 49·6	+1·26	...	29·5	7	...	−0·2
	1851·8	1845	29 12·11	+0·014	...	...	57·04	...	4	...	...
$R.C._2$	1855·7	1860	...	...	54 49·1	+1·26	...	31·1	6	...	−1·9
	1857·1	1860	28 49·67	+0·03	...	...	57·34	...	3	− 22	...
Arg.	1860·6	1860	28 49·27	+0·001	54 47·54	0·0	56·91	28·3	2	+ 27	+0·8
	1861·8	1864	28 43·09	+0·011	55 51·64	0·0	56·89	29·0	2	+ 31	0·0
H.C.	1865·7	1865	28 42·163	0·000	...	...	57·51	...	10	− 25	...
Arg.	1873·4	1873	28 29·51	+0·011	58 13·47	0·0	57·34	28·3	3	+ 4	+0·6
Main	1874·7	1874	28 28·17	+0·055	58 29·92	0·0	57·62	29·0	13	− 21	−0·2

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Additional Note concerning 3511 *Groombridge.*

Since writing my former Note upon this star's proper motion, I have received, by Professor Holden's kindness, the extracts from Struve's observations (1814–15) and Schwerd's Catalogue (Oeltzen) which relate to it. The following are the results:—

Catalogue.	Epoch. Obs.	Epoch. Pos.	R.A. at Epoch. h m s	Corr. s	Decl. at Epoch. ° ′ ″	Corr. ″	Position, 1855. h m s	Position, 1855. ° ′ ″	No. of Obs.	C−O R.A.	C−O Decl.
Str.	1814·7	1815	21 29 54·98	+0·209	...	...	21 28 56·38	...	7	+7	...
Schw.	1828·	1828	29 36·683	+0·043	79 46 20·6	0·0	56·60	79 53 29·2	3	+6	+0·5

It will be seen that Struve and Schwerd agree very nearly with the formula deduced in the previous Note; the discrepancies in Right Ascension, $+0^s\cdot07$ and $+0^s\cdot06$ respectively, are both less than $0''\cdot2$ of space, so that I have thought it unnecessary to make a new least-square solution.

Williamstown, Mass.,
September 26, 1877.

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Note on the Second Catalogue of Micrometrical Measurements of Double Stars made at the Temple Observatory, Rugby.

By J. M. Wilson and G. M. Seabroke.

In the forthcoming volume of the *Memoirs* (vol. xliii.) will appear the Second Catalogue of our Micrometrical Measurements of 398 Double Stars. By some accident the introductory observations that were sent with them were mislaid, and the fact was not discovered till the sheets were printed off. We should be glad of an opportunity of making a few remarks on them.

They consist chiefly of measures made in the years 1874, 5, 6, 7, with a few of older date, and include the last measures taken in the old Observatory. The temporary building was taken down in June last, and a new and permanent Observatory has been built, along with a curator's house, on a site purchased for this purpose by the governing body. An honorary curator and sub-curator of the Observatory have been added to the staff of the school.

The instrument and the micrometer are the same as those described in the introduction to the First Catalogue (*Memoirs*, vol. xlii), viz. Alvan Clark's 8¼-inch refractor, and Dollond's parallel wire micrometer. The observations were made, with few exceptions, by ourselves, with the frequent assistance of Mr. Percy Smith, F.C.S. (the present sub-curator), in measures of position, when any doubt or discrepancy showed itself. A magnifying power of about 400 was always used.

As in previous years, much of the very limited time at our disposal has been spent in miscellaneous observations on the Moon, planets, clusters, nebulæ, &c., and in interesting and instructing members of the school in astronomical matters.

It may be of interest to give the result of an analysis of our distance-measures and those of Dunèr and Mr. Gledhill, made by Otto Struve in the *Vierteljahrschrift der Astronomischen Gesellschaft* (12. Jahrgang, 2 Heft.). He finds

Gledhill	= Dunèr + 0″·151	from 108	comparisons.
Wils. & Seab.	= Dunèr + 0·143	„ 101	„
Struve	= Dunèr + 0·101	„ 324	„

But he points out that Struve's distances need a constant correction of +0″·06. Hence, to reduce the distance-measures by these observers to a uniform scale, the following corrections must be applied:—

Struve	+0″·06
Dunèr	+0·17 for stars below 12″.
Gledhill	−0·02
Wils. & Seab.	−0·03

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Otto Struve recommends us to separate our observations, so that it might be possible to ascertain our separate personal errors. We have, however, ascertained its amount from a considerable number of observations, viz. $W = S + 0''.1$, and this agrees very nearly with the amount ascertained by himself from the few separate observations published, viz. $W = S + 0''.15$. But, from a comparison of our own measures with those of others, we concluded that our errors were of opposite signs, and that the mean of our measures would not show any appreciable systematic error. This appears from Otto Struve's investigation to be the case. And on this ground we shall probably continue to give joint results, as being more trustworthy and more immediately available to the orbit computer.

We may add, moreover, that it is our opinion that a personal error in measures of this kind is very different in nature from those made in time observations; that it is masked in individual cases by accidental errors of far larger magnitude; and that it would be quite unsafe to apply it as a constant correction. Our present opinion is that a series of most careful measures of distance made by us on an easy star on a single fine night cannot be relied on to $0''.1$, and that the only way of getting greater accuracy is to multiply the nights of observation.

Otto Struve also expresses the wish that our distances should be given to hundredths of a second, and they are so given in the Second Catalogue. We are very grateful to him for his encouraging criticism, and his appreciation of our circumstances—"das Instrument in Rugby in grossem Maasse zur Befriedigung wissenschaftlicher Neugier dienen muss, und überdies die Beobachter durch ihre amtlichen Pflichten an der bekannten Lehranstalt vielfach in Anspruch genommen sind."

Rugby, Nov. 7, 1877.

On the Atmospheric Lines between the two D Lines.

By H. C. Russell, Esq., Director of the Sydney Observatory.

I have a powerful spectroscope made by Hilger, of London, of which the greatest dispersion is equal to eighteen 64° prisms, and the definition so fine that I can measure one three-hundredth part of the space between the D lines. With this I have frequently examined the lines between the D lines, and find only seven; while it is stated that at Potsdam nine are seen; and many years since (1868) Dr. Huggins saw twelve at Kew, and in 1875 Colonel Campbell recorded nineteen in London. These facts led me to make a careful examination and measures of all the lines I could see here. Perhaps the information may be interesting to the members of the Royal Astronomical Society.

The diagram has been prepared by enlarging the drawings kindly given to me by Dr. Huggins and Colonel Campbell, and a careful plot from my own measures. I may state that, in order

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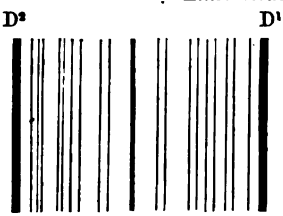
to identify the lines, I have numbered them 1 to 7, beginning at D1, and their positions, as determined by a number of readings with the micrometer, are as follows:—

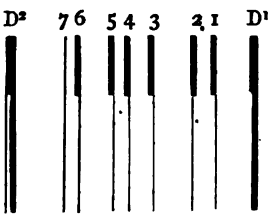
D1	1	2	3	4	5	6	7	D2	D3
000	41	67	124	151	173	213	232	291	299

4 is the nickel line always seen; 7 is the position, or very near it, of the zinc line; 1, 2, 3, 5, and 6 are evidently atmospheric lines, as they always increased in distinctness very fast as the Sun neared the horizon; 4 also seems to enlarge as if there were an atmospheric line coincident with it, and 6 increased faster than any other; when the Sun was near the horizon it was as thick again as D1; 7 did not increase at all, and almost disappeared when the Sun was near setting.

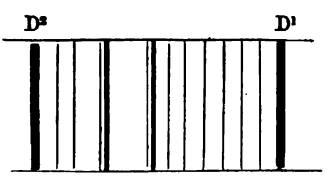
Lines between the "D Lines."



Colonel Campbell, 1875, London; with spectroscope by Hilger (eight glass prisms); "enlarged."



Sydney Observatory, 1877; spectroscope by Hilger (glass prisms). As thickened at the top, the lines show the appearance at sunset.



Dr. Huggins, 1863, with Mr. Gassiott's spectroscope at Kew, since presented to Oxford (bisulphide of carbon prisms); enlarged.

I have no drawing of the lines seen at Berlin.

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The line D3 has not, I think, been described before. It is a difficult line to see, and only to be made out with high powers. I have not reduced my measure to wave lengths, because the results obtained by Dr. Huggins and Colonel Campbell are only recorded in drawings, without measures. I have had prepared enlarged drawings of those obtained at Kew and London, with a careful plot from my own measures, which I enclose.

There are not many of the lines which agree; but as Dr. Huggins used bisulphide of carbon prisms, and Colonel Campbell only eight prisms, some of the differences might disappear if all could be reduced to wave lengths. On closely comparing the drawings, it will be seen that five lines were recorded in London between my No. 7 and D2, two others between D1 and No. 1, and three between Nos. 2 and 3. These spaces appear in Sydney, even under the most favourable conditions, entirely free from lines, or any sign of them. It would appear, therefore, that there must be some gases in the atmosphere of Europe, and especially of London, which are not present in Australia, or at least at Sydney.

Sydney Observatory,
July 26, 1877.

On the Spectrum of a Solar Spot observed at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

A fine spot has recently passed across the Sun's disk, showing a spectrum remarkable for the number of lines which are more or less affected by increased absorption, as will be seen from the following table:—

Element.	Number of lines observed.	Character of Change.
Calcium	12	Very much darker, and about doubled in breadth.
Sodium	2	Very much darker. The two D lines almost meet over the nucleus.
Titanium	11	Very much darker, and about doubled in breadth.
Iron	30	Slightly broader, by about a tenth, and decidedly darker, about half as dark again.
Barium	4	About twice as dark and twice as broad.
Magnesium	4	Much broader and darker.
Nickel	6	Half as broad again. Slightly darker.
Chromium	3	Somewhat darker, and broader by about one-tenth.
Hydrogen	3	Much fainter and less distinct. F line reversed in the neighbourhood of the spot, due north of the centre, on Nov. 5.
Coronal line	1	Much fainter over the spot.
D_3	1	Not seen either bright or dark, in spot or bridge.
Telluric bands	1	Band *a*. Broader and darker.

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The following lines and bands were also broadened :—

Line.	Wave length.	Character of Change.
K 588	6927	Darkened.
B	6869	Darkened and slightly broadened on the red side by about one tenth-metre of wave length.
K 712	6517	Double usual breadth. This is probably one of the lines of Barium.
K 765 ±	6380 ±	An appearance as of a dark shaded band, sharp towards the blue, and shaded off towards the red. Nothing seen on the Sun to correspond with it.
K 1493	5297	A cluster of several lines, of which only those on the blue side are affected. Much darker, and wider on the blue side by 0·3 tenth-metre of wave length. Two of these probably belong to Titanium.

Lines from Kirchhoff's map are denoted by the letter K and the No. on his scale.

Besides the increase in the breadth, displacements to the red or blue were noted, especially in the case of the F and D lines. On October 31 the F line was twisted, the part over the following half of the spot being displaced to the red, and the other half towards the blue, by amounts corresponding respectively to a recession or downward motion of nine miles, and an approach or upward motion of three miles a second. On the darkest part of the spot the D lines were broadened on the red side to the extent of $\frac{1}{2}(D_1 - D_2)$, the more refrangible edge being sharp, and not sensibly displaced. This would indicate a large downward motion of perhaps thirty miles a second. On other parts of the spot the haze appeared to be chiefly on the blue side. The spot was first seen (after an interval of cloudy weather) on October 30, when it was well advanced on the disk, and photographs were obtained on October 30, 31, November 1, 2, 3, 4, 5, during which period it underwent but little change, except that a group of small spots closely following it had closed up by November 5, leaving merely a group of faculæ, which were seen near the limb on November 8. The large spot would appear to have passed off about November $7^d\ 6^h$, but clouds prevented any observations between November 5 and 8. On November 8 there was no unusual disturbance of the chromosphere, the only indication of the position of the spot being a small jet 20″ high. With the exception of the group of faculæ the Sun appeared to be in a quiescent state.

The observations were made by Mr. Christie and Mr. Maunder, using the new "half-prism" spectroscope (direct-vision) with a train of two "half-prisms" reversed, so as to give great purity of spectrum.

Royal Observatory, Greenwich,
1877, November 9.

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Note on the Spectrum of the Eclipsed Moon. By Capt. W. Noble.

Those Fellows of the Society who were present on March 9 will remember that, at the conclusion of Mr. Penrose's Paper on the "Total Lunar Eclipse of February 27," I made some oral remarks on the spectrum of the obscured Moon as viewed from my own station. Notwithstanding the vivid redness of the lunar disk as seen by the naked eye, I found, to my exceeding surprise, that its spectrum was cut off in a most remarkable manner at the least refrangible end; in fact, that, with a pretty wide slit, it did not begin very much before D, as determined by an extemporised sodium-spectrum. I learned afterwards that no such truncation was visible at Greenwich. Observing the second eclipse of the Moon this year (that of August 23) I once more turned my spectroscope on to the shadowed disk, this time to be struck by the brilliancy of the red end of the spectrum, which seemed to me to extend to little *a*, if not further. Now, however, comes the curious part of the story. In the supplementary number of the *Monthly Notices* I find that Mr. Christie and Mr. Maunder, observing at Greenwich, noticed that "the red and blue ends of the spectrum were completely cut off, whilst the orange was greatly reduced in intensity." This would surely seem to point to our own atmosphere as the immediate cause of this remarkable absorption. Forest Lodge is only some thirty-seven or thirty-eight miles, as the crow flies, from the Royal Observatory, but this is an interval which would admit of very diverse atmospheric conditions obtaining at the two stations. It may be that the recent curious and interesting discovery of what he calls the "rainband" by the Astronomer Royal for Scotland (of which his own account will be found in recent Nos. of the *Astronomical Register*) will afford at least a partial explanation of the remarkable discrepancy. I find, on reference to my own meteorological register, that rain fell at my station on February 27, when the red end of the spectrum was cut off, and that none descended there on August 23, when the spectrum extended so much further. At all events, there seems no escape from the conclusion that the cause of the variation was terrestrial. Had it had its origin outside of our atmosphere, that is in the quality of the light refracted within the Earth's shadow, no difference could possibly have been perceptible at places only thirty-seven miles apart.

Physical Observations of Mars, made at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal.*)

The spectrum of *Mars* has been examined on several occasions lately with the single-prism spectroscope, in order to detect either

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traces of atmospheric absorption, or any differences in the spectrum as obtained from different parts of the disk. For the former purpose the spectrum of *Mars* was compared with that of the Moon on August 23 and September 26, when their altitudes were very nearly equal, that of *Mars* being 26° and 24° on the two occasions respectively, and of the Moon 24° and 23°. Several faint diffused bands, besides the sharp Fraunhofer lines, were observed in the spectrum of *Mars* on both occasions; and some of these were also seen on September 21, when the planet was again examined, but not compared with the Moon. The following table shows the wave-lengths of these bands, expressed in tenth-metres:—

Absorption Bands in Spectrum of Mars.

Line.		Aug. 23.	Sept. 21.	Sept. 26
Band δ of the Brewster spectrum	First edge	...	5640	5639
	Second edge	...	5661	5717
Group of lines round D	First edge	...	5889	5887
	Second edge	...	5907	5897
Faint band. Centre reading		6019	6022	...
Band *a*. Centre reading		6287	6287	6298
Very faint band. Centre reading		...	...	6511
Group of lines round C	First edge	6544	6537	6544
	Second edge	6572	6587	6575
Very faint band. Centre reading		...	...	6695
Very faint band B (?). Centre reading		6852	...	6895

Of these bands only the three most pronounced—viz. band *a* and the groups round C and D—were visible in the spectrum of the Moon. Of these *a* was only suspected to be present; the D group was decidedly narrower than in *Mars*, and that round C was only ⅔ the breadth of the same group in *Mars*, and so much fainter that it was not seen until the pointer had been set to the proper reading for it, although, when once it had been found, it was easily recognised. The dispersive power of the spectroscope employed being small, and the red end of the spectrum much contracted, the groups of atmospheric lines round C and D would necessarily appear as an unbroken band.

On September 12, when the large dark spot, called in Proctor's Chart "Dawes Ocean," occupied the centre of the disk, the spectrum of *Mars* was examined for the detection of local differences. The dark spot gave a much fainter spectrum than the rest of the disk, the contrast being very marked in the red and yellow, and less so in the violet; it showed, however, no lines or bands other than those seen in other parts. The spectrum of the polar spot and its neighbourhood was very bright from about D to F, but showed very little red; the north limb, on the

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contrary, gave a spectrum somewhat less bright in the green and yellow, but extending further into the red. The group round C seemed to be darker over "Dawes Continent" (the bright spot north of "Dawes Ocean") and less dark on the north limb. Bands *a* and B were seen, but no difference was remarked in their appearance from whatever part of the disk obtained; but, being fainter than C, a slight difference was less likely to be detected.

On September 21 and 26 the absorption bands then measured were noticed to be very slightly fainter on the limb than anywhere else. The locality where they were most distinct was variously estimated as from 5″ to 6″ from the limb on September 21, and from 3″ to 4″ on September 26. As the definition was much better on the latter occasion, the smaller estimation is more likely to be correct. The difference in intensity, except just on the limb, was exceedingly slight, but the point where the bands were estimated to be darkest was always found to be at very nearly the same distance from the limb.

The red end of the spectrum of *Mars* was much fainter on or near the limb, but the violet seemed of the same intensity from all parts of the disk, and was visible about as far as in the spectrum of the Moon.

The observations were made by Mr. Maunder.

Drawings (29 in all) have been made of the planet at every favourable opportunity during the late opposition, and some apparent changes, suggesting variations in the atmosphere of *Mars*, have been noted. It is, however, very difficult to determine how far such changes are due to the varying state of our own atmosphere. Variations were noted in the colour of the markings and in the comparative brightness of the limb, which usually presented the appearance of a bright ring, obliterating the dark markings more or less completely within about 4″ from the edge of the disk. The table shows the appearances that have been noted on various occasions, and the state of the definition corresponding.

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Changes in Appearance of Mars.

1877.	Aug. 30.	Sept. 12.	Sept. 19.	Sept. 21.	Sept. 24 & 26.	Sept. 27 & 29.	Oct. 2.	Oct. 15.	Oct. 16.	Oct. 22.	Oct. 23.
Bright ring round limb.	Bright.	Bright.	Hardly visible; limb orange.	More apparent, but not so bright as on Sept. 12.	Not perceptible.	More marked.	Very marked.	Very marked.	Scarcely visible; dark markings visible up to limb.	Not marked.	Very bright on *p* side.
Dark spots.	Blue gray.	Dark olive green.	Neutral.	...	...	...	Neutral.	...	...	Dusky.	Cobalt blue.
Bright spots.	Rose.	...	Dull orange.	Yellower or whiter than on Sept. 19.	Orange; purer and livelier than on Sept. 19.	...	Livelier orange, brightening in parts into yellow or white.	...	...	Colour not marked.	Colour very marked; pale vermilion.
Definition.	...	Very good.	...	Trem. and bad.	Much better on Sept. 26.	Sept. 27, poor; Sept. 29, good.	Good, and very steady.	Bad, and unsteady.	Faint from fog, and not steady.	Good.	Very good.

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The following points have also been noted:—On September 12 a thick cluster of points, darker than the bright spot but much brighter than the adjoining dark spot, formed a fringe on the border between Dawes Continent and Dawes Ocean (Proctor's Chart) or Hour-glass Sea, a boundary which, when seen under nearly the same aspect on September 7, had appeared remarkably sharp and distinct. On October 15 the long narrow bright strip stretching into the centre of the disk was very ill-defined at both ends, where it was both fainter and whiter than the rest. A very bright white spot was noted on October 22 on the *s.p.* limb in a position corresponding to the *p.* end of this strip. The large bright patch towards the *s.f.* limb (Lockyer Land of Proctor's Chart) seemed to move round a little more rapidly than the other markings, as if elevated above them. In the course of a couple of hours it seemed to have encroached on the dark strip on its *n.p.* side, making it appear narrower. The bright patch (Lockyer Land) was noted as being very bright and white on October 22. Dawes' so-called "Snow Island" was seen on October 2 as a white spot in De la Rue Ocean.

On August 30 the polar spot appeared to be serrated, showing three indentations round the edge when the definition was at its best. As far as could be judged, this was not an effect of tremor in the air. On September 29 the polar spot was noted as being irregular in shape, and on October 22 the dark ring round it appeared to be much darker on the *f.* side.

The above observations would appear to indicate the presence of occasional clouds in the atmosphere of *Mars*, but they require confirmation.

The outer satellite of *Mars* was seen on five nights and the inner on one, viz. September 28. Only one micrometer measure of the outer satellite was obtained, as follows:—

1877.	G.M.T. h m s	Pos. Angle. ° ′	Dist. (from 1st limb). ″
Sept. 29	8 54 56	248 31	67·5

Estimations were made on the other nights.

The observations were made by Mr. Christie and Mr. Maunder.

Royal Observatory, Greenwich,
1877, *November* 9.

Observations of Mars at Madeira, August and September 1877.
By N. E. Green, Esq.

Madeira was chosen as a place of observation for its southern position, its reputation for clear skies during the months of August and September, and because the heat at that season is less than at other places on the same parallel of latitude.

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Observations were commenced on August 19 at a situation to the east of Funchal, having an elevation of 1,200 feet above the sea, and continued till September 14, when a move was made to a position a thousand feet higher, with the hope of improving the definition; but the change was not attended with any decisive advantage.

The great transparency of the atmosphere at Madeira encouraged the expectation of a corresponding degree of sharpness in the telescopic images; but its want of steadiness, arising apparently from conflicting currents of hot and cold air, seriously affected the performance of the instrument, and frequently restricted the amount of amplification that could be employed with advantage. Still there were many good evenings, and amongst them some which merited the term superb.

The following extract from the notes taken on the occasion will enable those interested in the matter to form their own estimate of the value of this situation for astronomical studies. There were altogether 47 nights during which the telescope was ready for work. Out of this number 26 were sufficiently favourable for the use of the pencil, leaving 21 on which no drawings could be made. Of the 26 possible occasions, 10 are entered as good, 4 as excellent, and 2 as grand and superb, the remaining 10 evenings being either passable or poor. They may be tabulated thus:—

Total number of nights	47
Number favourable for drawing	26
Wet, cloudy, or useless	21
	47
Of the 26 favourable nights—	
Entered as good	10
„ excellent	4
Grand and superb	2
Passable or poor	10
	26

Thus out of 47 nights there were 16 of good character, giving a proportion of one-third, a quantity considerably in excess of the average of an English climate. The instrument employed was a 13-inch silver-on-glass reflector, mounted as an altazimuth, its construction being of the most simple character, and so arranged that it could be readily taken to pieces and carried up the steep roads of the island. Its situation was changed four times, and each time it was removed and erected within the day, so that no night was lost.

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The 13-inch mirror was figured by Mr. G. Wills, of Hereford, and most kindly lent by him for the occasion. Its performance either on planet or stars was simply perfect, the higher powers having apparently no effect on its definition. A very fine 12½-inch speculum by Messrs. Horne & Thornthwaite was also employed; and it may be worth the remark to those who are interested in these silver-on-glass mirrors, that the quality of their performance seemed to leave nothing to desire.

The method of mounting may be observed in the photographs which lie on the table. It will be seen that the framework which holds the mirrors in position is open to the external air, excepting at that portion which is nearest the observer; here it is partly closed in order to exclude any currents which might be formed by heat arising from the person. By this open arrangement the tube currents which so generally impair the performance of reflectors are impossible.

A Paper on this subject is in course of preparation, in which the results of the working of a 4-inch achromatic, a 9-inch reflector in the usual iron tube, and a 13-inch speculum mounted in an open frame are carefully registered, showing a most decided advantage in the open arrangement over the usual iron tube.

The drawings, 41 in number, were continued until the whole surface of the planet had passed before the eye, the same phase that had occupied the pencil at an advanced hour of the night on August 19 having returned to the meridian early in the evening of September 19. Drawings were continued till September 29, thus overlapping the series, and producing duplicates of many of the earlier views. Of these views twelve, which were produced on the most favourable occasions, have been carefully copied, and presented for the acceptance of the Society.

Each drawing was made direct from the telescope, and entirely independent of those which had been produced previously; all comparisons being reserved till the evening was over, so that each view might be as free as possible from bias, or a leaning towards the repetition of similar forms. Occasionally the planet was scrutinised most carefully, in order to verify, if possible, the drawings of others, and especially the maps published by Mr. Proctor from the drawings of Mr. Dawes. Attention was also given to the work of M. Terby, and an earnest effort made to answer each query of that painstaking astronomer.

The drawings furnish evidence of the general clearness of the atmosphere of *Mars*, the various details being fairly repeated from night to night, even after the retrograde movement due to the greater length of the Martial day had brought the same phase again to the meridian. The presence of an atmospheric envelope is, however, abundantly shown in the fading both of details and colour towards the limb, and especially from the singular absence of markings towards the north pole, where, in consequence of a lower temperature and shorter exposure to the Sun's rays, a greater amount of condensation would take place.

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In connection with atmospheric effects, a most interesting observation was made on the evening of August 21. On this occasion a series of lines having a meridional direction were seen tending towards the north pole, possibly indicating the flow of currents of cold air to the equator; and on September 29 one of the most firm and definite lines on the planet, the eastern edge of the De la Rue Ocean, was in two places obscured by clouds.

The decrease of the polar zone of snow was most manifest, as in some drawings made in St. John's Wood previous to the voyage to Madeira this zone occupies a space fully twice as great as in those made towards the end of September. A most interesting observation was made on September 1. At 10.40 a brilliant spot of light was seen at a considerable distance from the edge of the snow, on the western portion of the ellipse; this was clearly seen again on the 2nd and 3rd of the month; and on the 8th, during which night the definition was superb, a second point was visible near the first; and again, on September 10, a series of fainter points were figured, forming a line concentric with the zone of snow. These isolated lights were doubtless due to the presence of high ground, which retained its covering of snow after it had disappeared from the lower surfaces. The outline of the zone was also broken up by dark valleys extending into it, and several irregular patches formed projections, which a further contraction might have left as outlying fields of snow.

It has been remarked that these snow caps do not agree exactly with the poles of the axis of revolution. It may be a confirmation of this statement to mention that by September 18, the zone, only a week after it had appeared so large and distinct, was considerably reduced in size and its outline very indefinite, whereas by the end of the month it had returned in great measure to its previous form and size; the rapidity and extent of this change being greater than could be attributed to simple alterations of temperature.

The portions of the planet's surface where the details observed seem least to agree with the drawings made during previous oppositions is that to the east of the De la Rue Ocean. Here are figured in Mr. Proctor's maps, and in the drawings of some eminent observers, two very similar forms called Lockyer Sea and Dawes Sea. Now the Lockyer Sea is perhaps the most definite separate dark spot on the planet. It was observed and figured at Madeira 18 times, but on no occasion could the Dawes Sea be made out. There is a very delicate shade proceeding from a more northern portion of the De la Rue Ocean, somewhat in the position where this sea is figured, and this may have been darker on previous oppositions, or the delicate shading of the original drawings may have been exaggerated either by engraver or printer.

The Lockyer Sea is generally represented as united to the De la Rue Ocean by a narrow inlet, and on one occasion it had

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just this appearance at Madeira; but on the same evening, in steadier air and under a higher power, this appearance was found to be due to the presence of a small lake lying in the line of the supposed connection.

A most careful search was frequently made for the long snakelike form called Bessel Inlet; but, though the examination was continued on a favourable occasion for two hours, it was in vain.

The forms of the markings generally considered to exist both to the west and east of the Kaiser Sea are capable of improvement. They are very delicate, the finest air and considerable magnifying power being required for their observation.

The most earnest attention was given to the delineation of the south pole and its surroundings, in order to make the most of this memorable opposition, and there appears to be very strong evidence of the existence of a sea lying between the southern portion of Hook's Sea and that known as Phillips' Sea, which immediately surrounds the pole.

I cannot conclude this Paper without expressing my thanks to Mr. Marth, who has taken so great an interest in this effort, furnishing me, in the first place, with every information about the island, and afterwards sending me most valuable tables of the revolution of *Mars*, and the dates when certain markings would be on the meridian. I only regret that, in consequence of the close occupation of my time in making the drawings of *Mars*, and the weariness of body produced by work both by day and night, that I was not able to observe the satellites of *Saturn*, as that indefatigable computer would have desired. Still it is hoped that the Madeira observations will not be without result in advancing our knowledge of the most terrestrial of the heavenly bodies.

It is intended to add a critical examination of each of the drawings, together with several matters of interest connected with the observations; but these are more fitted to appear together with the publication of the views of the planet.

Sketches of Mars. By Capt. W. Noble.

The sketches of *Mars*, which I herewith present to the Society, almost explain themselves. They were all made with my (Ross) Equatoreal of 4·2 inches aperture and sixty-one inches focus, and with the same power of 255. I was never able to make out any markings on or near the northern limit of the planet; and, as a rule, all markings shaded off as they approached the preceding and following limbs. It will be noticed, however, that in the greater proportion of cases the markings are sharper and darker towards the following limb than towards the preced-

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ing one, and in the not very frequent instances in which a dark marking is seen to extend actually *to* the limb itself, it is invariably the following one. This would seem to point to the fact that sunrise on *Mars* is much clearer than sunset; but I abstain from any theory as to the cause of this.

Forest Lodge, Maresfield, Uckfield,
November 8, 1877.

On the Motion of the Moon's Node in the case when the Orbits of the Sun and Moon are supposed to have no Eccentricities, and when their mutual Inclination is supposed to be indefinitely small. By Professor J. C. Adams, M.A.

A very able paper has recently been published by Mr. G. W. Hill, assistant in the office of the *American Nautical Almanac*, on the part of the motion of the lunar perigee which is a function of the mean motions of the Sun and Moon.

Assuming that the values of the Moon's coordinates in the case of no eccentricities are already known, the author finds the differential equations which determine the inequalities which involve the first power of the eccentricity of the Moon's orbit, and, by a most ingenious and skilful process, he makes the solution of those differential equations depend on the solution of a single linear differential equation of the second order, which is of a very simple form. This equation is equivalent to an infinite number of algebraical linear equations, and the author, by a most elegant method, shows how to develop the infinite determinant corresponding to these equations in a series of powers and products of the small quantities forming their coefficients. The value of the multiplier of each of such powers and products as are required is obtained in a finite form. By equating this determinant to zero, an equation is obtained which gives directly, and without the need of successive approximations, the motion of the Moon from the perigee during half of a synodic month. The small quantities which enter into the value of the above determinant are of the fourth, eighth, twelfth, &c. orders, considering, as usual, the ratio of the mean motion of the Sun to that of the Moon as a small quantity of the first order; and the author has taken into account all the terms of lower orders than the sixteenth. The ratio of the motion of the perigee to that of the Moon thus obtained is true to twelve or thirteen significant figures. The author compares his numerical result with that deduced from Delaunay's analytical formula, which gives the ratio just mentioned developed in a series of powers of m, the ratio of the mean motions of the Sun and Moon. The numerical

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coefficients of the successive terms of this series increase so rapidly that the convergence of the series is slow, so that the terms calculated do not suffice to give the first four significant figures of the result correctly, although, by induction, a rough approximation may be made to the sum of the remaining terms of the series.

I have been led to dwell thus particularly on Mr. Hill's investigation because my own researches in the Lunar Theory have followed, in some respects, a parallel course, *sed longo intervallo*.

I have long been convinced that the most advantageous way of treating the Lunar Theory is, first, to determine with all desirable accuracy the inequalities which are independent of the eccentricities e and e', and the inclination $2 \sin^{-1}\gamma$, and then, in succession, to find the inequalities which are of one dimension, two dimensions, and so on, with respect to those quantities.

Thus the coefficient of any inequality in the Moon's coordinates would be represented by a series arranged in powers and products of e, e', and γ, and each term in this series would involve a numerical coefficient which is a function of m alone and which may be calculated for any given value of m without the necessity of developing it in powers of m. The variations of these coefficients which would result from a very small change in m might be found either independently or by making the calculation for two values of m differing by a small quantity.

This method is particularly advantageous when we wish to compare our results with those of an analytical theory such as Delaunay's, in which the eccentricities and the inclination are left indeterminate, since each numerical coefficient so obtained could be compared separately with its analytical development in powers of m.

It is to be remarked that it is only the series proceeding by powers of m in Delaunay's Theory which have a slow rate of convergence, so that it is probable that all the sensible corrections required by Delaunay's coefficients would be found among the terms of low order in e, e', and γ.

The differential equations which would require solution in these successive operations after the determination of the inequalities independent of eccentricities and inclination would be all linear and of the same form.

It is many years since I obtained the values of these last-named inequalities to a great degree of approximation, the coefficients of the longitude expressed in circular measure, and those of the reciprocal of the radius vector, or of the logarithm of the radius vector, being found to ten or eleven places of decimals.

In the next place I proceeded to consider the inequalities of latitude, or rather the disturbed value of the Moon's coordinate perpendicular to the ecliptic, omitting the eccentricities as before, and taking account only of the first power of γ.

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In this case the differential equation for finding z presents itself naturally in the form to which Mr. Hill reduces, with so much skill, the equations depending on the first power of the eccentricity of the Moon's orbit.

In solving this equation I fell upon the same infinite determinant as that considered by Mr. Hill, and I developed it in a similar manner in a series of powers and products of small quantities, the coefficient of each such term being given in a finite form.

The terms of the fourth order in the determinant were thus obtained by me on the 26th December 1868. I then laid aside the further investigation of this subject for a considerable time, but resumed it in 1874 and 1875, and on the 2nd of December in the latter year I carried the approximation to the value of the determinant as far as terms of the twelfth order, or to the same extent as that which has been attained by Mr. Hill. I have also succeeded in reducing the determination of the inequalities of longitude and radius vector which involve the first power of the lunar eccentricity to the solution of a differential equation of the second order, but my method is much less elegant than that of Mr. Hill.

Immediately after Mr. Hill's paper reached me, I wrote to him expressing my opinion of its merits, and telling him what I had done in the same direction, and I received from him a very cordial and friendly letter in reply.

The equation which I had obtained by equating the above-mentioned determinant to zero differed in form from Mr. Hill's, and on making the reductions required to make the two results immediately comparable, I found that there was an agreement between them except in one term of the twelfth order. On examining my work I found that this arose from a simple error of transcription in a portion of my work, and that when this had been rectified my result was in entire accordance with Mr. Hill's.

The calculations by which I have found the value of the determinant are very different in detail from those required by Mr. Hill's method, and appear to be considerably more laborious. I have not yet had time to copy out and arrange the details of the calculations from my old papers, but I hope soon to do so, thinking that they may not be without interest for the Society. Meantime I now make known the result which I have obtained for the motion of the Moon's node on the suppositions stated in the title of this paper.

If nt and $n't$ represent the mean longitudes of the Moon and the Sun at time t, omitting, for the sake of brevity in writing, the constants which always accompany nt and $n't$, and if θ and r represent the Moon's longitude and radius vector, I find that, in the case of no eccentricities and inclination, if $m = \frac{n'}{n} = 0{\cdot}0748013$, which is the value used by Plana,

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$$\theta = nt + 0{\cdot}01021{,}13629{,}5 \sin 2(n-n')t$$
$$+ 0{\cdot}00004{,}23732{,}7 \sin 4(n-n')t$$
$$+ 0{\cdot}00000{,}02375{,}7 \sin 6(n-n')t$$
$$+ 0{\cdot}00000{,}00015{,}1 \sin 8(n-n')t$$
$$+ 0{\cdot}00000{,}00000{,}1 \sin 10(n-n')t;$$

$$\frac{1}{r} = 1{\cdot}00090{,}73880{,}5$$
$$+ 0{\cdot}00718{,}64751{,}6 \cos 2(n-n')t$$
$$+ 0{\cdot}00004{,}58428{,}9 \cos 4(n-n')t$$
$$+ 0{\cdot}00000{,}03268{,}6 \cos 6(n-n')t$$
$$+ 0{\cdot}00000{,}00024{,}3 \cos 8(n-n')t$$
$$- 0{\cdot}00000{,}00000{,}3 \cos 10(n-n')t;$$

supposing that θ is expressed in the circular measure, and that the unit of distance is the mean distance in an undisturbed orbit which would be described by the Moon about the Earth in the same periodic time. In this case, if μ denote the sum of the masses of the Earth and Moon, we shall have

$$\mu = n^2.$$

The differential equation which determines z, the Moon's coordinate perpendicular to the Ecliptic, is

$$\frac{d^2z}{dt^2} + \left(\frac{\mu}{r^3} + \frac{m'}{r_1^3}\right) z = 0.$$

Now, the Sun's orbit being circular, we have $\frac{m'}{r_1^3} = n'^2$, and the only function of the Moon's coordinates which we require in order to form this equation is $\frac{1}{r^3}$.

I find that, with the above unit of distance,

$$\frac{1}{r^3} = 1{\cdot}00280{,}21783{,}115$$
$$+ 0{\cdot}02159{,}98364{,}4 \cos 2(n-n')t$$
$$+ 0{\cdot}00021{,}53273{,}9 \cos 4(n-n')t$$
$$+ 0{\cdot}00000{,}20644{,}8 \cos 6(n-n')t$$
$$+ 0{\cdot}00000{,}00192{,}9 \cos 8(n-n')t$$
$$+ 0{\cdot}00000{,}00000{,}3 \cos 10(n-n')t.$$

Let

$$\frac{1}{(n-n')^2}\left(\frac{\mu}{r^3} + \frac{m'}{r_1^3}\right), \text{ or } \frac{1}{(n-n')^2}\left(\frac{n^2}{r^3} + n'^2\right), = \frac{1}{(1-m)^2}\left(\frac{1}{r^3} + m^2\right),$$

$$= q^2 + 2q_1 \cos 2(n-n')t + 2q_2 \cos 4(n-n')t + 2q_3 \cos 6(n-n')t + \&c.;$$

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then we find, from the above value of $\frac{1}{r^3}$, that

$$q^2 = 1{\cdot}17804,44973,149, \quad \text{and} \quad q = 1{\cdot}08537,75828,323,$$
$$q_1 = 0{\cdot}01261,68354,6,$$
$$q_2 = 0{\cdot}00012,57764,3,$$
$$q_3 = 0{\cdot}00000,12059,0.$$

These are all the quantities necessary for finding the motion of the Moon's node, to the order which we require.

If $g\pi$ denote the angular motion of the Moon from its node in half a synodic period of the Moon, the equation so often referred to above gives

$$\cos g\pi = \cos q\pi \left\{ 1 - \frac{\pi^2 q_1^4}{32q^2(q^2-1)^2} - \frac{15q^4-35q^2+8}{256q^4(q^2-1)^4(q^2-4)}\pi^2 q_1^6 + \frac{3\pi^2 q_1^4 q_2}{32q^2(q^2-1)^2(q^2-4)} - \frac{\pi^2 q_1^2 q_2^2}{16q^2(q^2-1)(q^2-4)} \right\}$$
$$+ \sin q\pi \left\{ \frac{\pi q_1^2}{4q(q^2-1)} + \frac{15q^4-35q^2+8}{64q^3(q^2-1)^3(q^2-4)}\pi q_1^4 - \frac{\pi^3 q_1^6}{384q^3(q^2-1)^3} + \frac{105q^{10}-1155q^8+3815q^6-4705q^4+1652q^2-288}{256q^5(q^2-1)^5(q^2-4)^2(q^2-9)}\pi q_1^6 \right.$$
$$- \frac{3\pi q_1^2 q_2}{8q(q^2-1)(q^2-4)} - \frac{35q^6-280q^4+497q^2-108}{32q^3(q^2-1)^3(q^2-4)^2(q^2-9)}\pi q_1^4 q_2$$
$$+ \frac{\pi q_2^2}{4q(q^2-4)} + \frac{15q^6-110q^4+179q^2-36}{16q^3(q^2-1)^2(q^2-4)^3(q^2-9)}\pi q_1^2 q_2^2$$
$$\left. + \frac{\pi q_2^2}{4q(q^2-9)} - \frac{(3q^2-7)\pi q_1 q_2 q_3}{4q(q^2-1)(q^2-4)(q^2-9)} + \frac{5\pi q_1^2 q_3}{16q(q^2-1)(q^2-4)(q^2-9)} \right\}$$

Now, if the coefficients of cos $q\pi$ and sin $q\pi$ in this formula be converted into numbers, employing the above values of q, q_1, &c., we find

$$\cos g\pi = \cos q\pi\ [0{\cdot}99999,97902,01654] + \sin q\pi\ [0{\cdot}00064,77652,06681].$$

But, with the above value of q, we find, from Briggs' Tables,

$$\cos q\pi = -0{\cdot}96424,37306,84295$$
$$\sin q\pi = -0{\cdot}26501,70331,05484.$$

Hence

$$\cos g\pi = -0{\cdot}96441,51972,00779.$$

Whence, by the same Tables, we find that

$$g = 1{\cdot}08517,13927,46869.$$

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and therefore the ratio of the Moon's motion from the node to its sidereal motion is

$$g(1-m) = 1{\cdot}00399,91618,46592.$$

This is the quantity ordinarily denoted by g in the Lunar Theory.

Delaunay's value of g, which agrees with that of Plana, is

$$g = 1 + \frac{3}{4}m^2 - \frac{9}{32}m^3 - \frac{273}{128}m^4 - \frac{9797}{2048}m^5 - \frac{199273}{24576}m^6 - \frac{6657733}{589824}m^7.$$

If this be converted into numbers by substituting the value of $m = 0{\cdot}0748013$, we find

$$g = 1{\cdot}00399,91722,8,$$

which differs from the true value in the eighth place of decimals.

If we take $\mathrm{m} = \frac{m}{1-m}$ and develop the value of g in powers of m, we find

$$g = 1 + \frac{3}{4}\mathrm{m}^2 - \frac{57}{32}\mathrm{m}^3 + \frac{123}{128}\mathrm{m}^4 - \frac{1925}{2048}\mathrm{m}^5 + \frac{25667}{24576}\mathrm{m}^6 - \frac{268309}{589824}\mathrm{m}^7;$$

and substituting the value of

$$\mathrm{m} = 0{\cdot}08084,89030,52,$$

we find

$$g = 1{\cdot}00399,91591,1,$$

which is considerably nearer the truth than the value found from the series in powers of m.

The numerical values of the successive terms of the series for $g-1$, in terms of powers of m and of m respectively, are given in the following comparative table :—

In powers of m.			In powers of m.		
m^2		·00419,64258,6	m^2		·00490,24088,4
m^3	−	11,77117,9	m^3	−	94,13416,4
m^4	−	6,67712,1	m^4		4,10574,2
m^5	−	1,12023,4	m^5	−	,32469,2
m^6	−	,14203,4	m^6		,02916,8
m^7	−	,01479,0	m^7	−	,00102,7
		·00399,91722,8			·00399,91591,1

This shows that the development in powers of m is much more advantageous than that in powers of m.

The same thing likewise holds good with respect to the value of c, which determines the motion of the perigee.

The following is a similar table, showing the numerical values

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of the successive terms of Delaunay's series for $1-c$ in powers of m, and of the terms of the corresponding series in powers of m :—

In powers of m.		In powers of m.	
m^2	·00419,64258,6	m^2	·00490,24088
m^3	294,27947,8	m^3	292,31135
m^4	99,56981,8	m^4	55,37745
m^5	30,35769,9	m^5	14,37162
m^6	9,13946,6	m^6	3,49278
m^7	2,82999,6	m^7	,99062
m^8	,98356,5	m^8	,42111
m^9	,34684,2	m^9	,08515
	·00857,14945,0		·00857,29096

The true value reduced from Mr. Hill's, so as to correspond to the value of m which we have employed, is

·00857,25645.

Hence, as in the former case, the advantage of developing in powers of m is very evident.

I have found that a similar advantage results from the employment of m instead of m in the development of the co-efficients of the Moon's periodic inequalities.

On a New Term of Long Period in the Mean Motion of the Moon.

By E. Neison, Esq.

The mean motion of the Moon contains a term due to the direct action of the planet *Mars*, and possessing the argument

$$\{(n-24n''+20n''')\,t+\mathrm{E}\},$$

where, following Leverrier's nomenclature, two accents denote quantities belonging to the Earth, and three accents those belonging to the planet *Mars*. The following note is directed to determining provisionally the value of this term :—

Let $\mathrm{R}^{(''')}$ denote the disturbing function arising from the direct action of *Mars* upon the Moon. Then, as shown by Delaunay,

$$\mathrm{R}^{(''')} = m'''\left\{\frac{r^2}{\Delta^3}\left(\frac{3}{2}\cos^2\mathrm{H}-\frac{1}{2}\right)+\frac{r^3}{\Delta^4}\left(\frac{5}{2}\cos^3\mathrm{H}-\frac{3}{2}\cos\mathrm{H}\right)\right\},$$

where

Δ = geocentric distance of *Mars*,

H = geocentric angular distance between *Mars* and the Moon.

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Suppose the inclinations of the Moon and planet to be neglected, which can be done without sensibly affecting the results, then

$$\cos H = \frac{r'''}{\Delta}\cos(v-v''') - \frac{r''}{\Delta}\cos(v-v''),$$

whence

$$\cos^2 H = \frac{1}{\Delta^2}\left\{\left(\frac{3}{4}r'''^2 + \frac{3}{2}r''^2 r'''\right)\cos(v-v''') - \left(\frac{3}{4}r''^2 + \frac{3}{2}r'''^2 r''\right)\cos(v-v'')\right.$$
$$\left. - \frac{3}{4}r''^2 r''\cos(v-2v''' + v'') + \frac{3}{4}r''^2 r'''\cos(v-2v'' + v''')\right\}.$$

Therefore, by substitution

$$R^{(m)} = m''' r^3\left\{\left(\frac{15}{8}\cdot\frac{r'''^2 + 2r''' r''^2}{\Delta^7} - \frac{3}{2}\frac{r'''}{\Delta^5}\right)\cos(v-v''') - \left(\frac{15}{8}\frac{r''^2 + 2r'' r'''^2}{\Delta^7} - \frac{3}{2}\frac{r''}{\Delta^5}\right)\cos(v-v'')\right.$$
$$\left. + \frac{15}{8}\frac{r'' r'''}{\Delta^7}\left[r''\cos(v-2v''+v''') - r'''\cos(v-2v'''+v'')\right] + \ldots.\right.$$

Throughout only terms depending on v have been retained.

As Δ is the geocentric distance of *Mars*,

$$\Delta^{-s} = \{r'''^2 + r''^2 - 2r'' r'''\cos(v''-v''')\}^{-\frac{s}{2}},$$

a well-known function.

Let

$$(a''')^{-s}\,[B_{\frac{s}{2}}]^i_{i+k}\cos\{[in'' - (i+k)n'''\,]\,t + E\}$$

be the general term of Δ^{-s}, a term which is obviously of the k-th order of the eccentricities.

Then, neglecting powers and products of the eccentricities of the Earth and *Mars*, and

$$\{r'''^2 + 2r''' r''^2\} = (a''')^3\{(1+2a^2+3e'''^2) - (3+2a^2)e'''\cos\alpha'''$$
$$+ \ldots. - 4a e''\cos\alpha'' + \ldots.\},$$

$$\{r''^2 + 2r'' r'''^2\} = (a''')^3\{(2+\ a^2+3e'''^2)\,a - 2a\,e'''\cos\alpha'''$$
$$+ \ldots. - (2+3a^2)\,a\,e''\cos\alpha'' + \ldots.\},$$

$$\cos(v-v''') = \cos(v-n'''t-\epsilon''') + \{2e'''\sin\alpha''' + \ldots.\}\sin(v-n'''t-\epsilon'''),$$
$$\cos(v-v'') = \cos(v-n''t-\epsilon'') + \{2e''\sin\alpha'' + \ldots.\}\sin(v-n''t-\epsilon''),$$

$$\cos(v-2v''+v'') = \cos(v+[\,n''-2n'''\,]\,t + \epsilon''-2\epsilon'')$$
$$+ \{4e'''\sin\alpha'''-2e''\sin\alpha'' + \ldots.\}\sin(v+[\,n''-2n'''\,]\,t + \epsilon''-2\epsilon'''),$$

$$\cos(v-2v''+v''') = \cos(v-[2n''-n'''\,]\,t-2\epsilon''+\epsilon''')$$
$$+ \{4e''\sin\alpha''-2e'''\sin\alpha''' + \ldots.\}\sin(v-[2n''-n'''\,]\,t+2\epsilon''-\epsilon'''),$$

where, for convenience, $\frac{a''}{a'''}$ has been denoted by α, as usual, and α'' and α''' denote the mean anomalies of the Earth and *Mars*.

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Further, if σ be the ratio of the masses of the Sun and *Mars*, and $m = \frac{n''}{n}$,

$$m'' = \sigma m^2 \left(\frac{a''}{a}\right)^3.$$

Substituting these values in the expression for $R^{(m)}$, it becomes

$$R^{(m)} = \sigma m^2 . a^4 . \left(\frac{a}{a''}\right) . \frac{r^3}{a^4} . \left\{ \frac{15}{16} \left([1 + 2\alpha^2 + 3e'^2] [B_{\frac{1}{2}}]^{24}_{21} - [2 + \alpha^2 + 3e'^2] \alpha [B_{\frac{1}{2}}]^{23}_{20} \right.\right.$$

$$\left. + \alpha^2 [B_{\frac{1}{2}}]^{22}_{19} - \alpha [B_{\frac{1}{2}}]^{25}_{22} \right.$$

$$- \frac{1}{2} (1 - 2\alpha^2) e'' [B_{\frac{1}{2}}]^{24}_{22} + \alpha e'' [B_{\frac{1}{2}}]^{23}_{21} - \frac{3}{2} e'' \alpha^2 [B_{\frac{1}{2}}]^{22}_{20} - e'' \alpha [B_{\frac{1}{2}}]^{25}_{23}$$

$$- 2\alpha e'' [B_{\frac{1}{2}}]^{23}_{21} - \frac{1}{2} (2 - \alpha^2) \alpha e'' [B_{\frac{1}{2}}]^{22}_{20} + \frac{3}{2} \alpha e' [B_{\frac{1}{2}}]^{24}_{22} + \alpha e' [B_{\frac{1}{2}}]^{21}_{19}$$

$$\left. + \frac{4}{5} [B_{\frac{1}{2}}]^{23}_{20} \alpha - \frac{4}{5} [B_{\frac{1}{2}}]^{24}_{21} \right) \cos (v - [24n'' - 20n'''] t - E) + \ldots .$$

The products of the eccentricities of *Mars* and the Earth, with the coefficients $[B_{\frac{1}{2}}]^{i}_{i+k}$, have been neglected because they will be very small.

In this expression, r^3 and v can be directly replaced by a''^3 and $nt + \epsilon$, as the terms depending on the eccentricity of the Moon, are not required. Then, putting the above coefficient in brackets equal to (R), we have

$$R^{(m)} = \sigma m^2 . a^4 . \left(\frac{a}{a''}\right) . \frac{1}{a} . (R) \cos \{[n - 24n'' + 20n'''] t + \epsilon - E\}.$$

Substituting this value in the equation for δn, since

$$\delta n = -3 \iint a n^2 \left(\frac{dR}{d\epsilon}\right) dt^2,$$

the inequality in the mean motion of the Moon will be

$$\delta n = -\sigma m^2 . a^4 . \left(\frac{a}{a''}\right) . \left(\frac{n}{n - 24n'' + 20n'''}\right)^2 . 3 (R) . \sin \{[n - 24n'' + 20n'''] t + \epsilon - E\}.$$

Adopting Hansen's value for the mean motion of the Moon (*Tables de la Lune*) and Leverrier's values for the mean motions of *Mars* and the Earth (*Annales de l'Observatoire*), then

n	= 17	325	594″·0
$24n'' - 20n'''$	= 17	322	447″·5
$n - 24n'' + 20n'''$	=	3	146″·5

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The motion of the perihelions of *Mars* and the Earth may be neglected in what follows, though tending to slightly decrease the results.

Suppose σ, or the ratio of the masses of *Mars* and the Sun, to be taken as $\frac{1}{3{,}000{,}000}$, and assume $\frac{a}{a''} = 0{\cdot}0025856$, corresponding to a solar parallax of $8''{\cdot}85$; then

$$\sigma m^2 a^4 \cdot \left(\frac{a}{a''}\right) \cdot \left(\frac{n}{n-24n''+20n'''}\right)^2 \cdot 3 = 12''{\cdot}5150 \,.\, \log = 1{\cdot}09753.$$

The coefficients $[B_i]^i_{i+k}$ can be readily calculated by making use of the developments given in either Pontécoulant's *Système du Monde*, or Delaunay's memoir in the *Conn. des Temps*, 1863.

On substituting the proper values in the above expression for (R), the first four terms entirely destroy each other, and with them vanishes the largest part of the coefficient of the term. The terms depending on the eccentricity of *Mars* yield therefore the principal part of the coefficient, those depending on the eccentricity of the Earth being decreased by its smallness. The last two terms destroy each other. The complete value is therefore

$$\begin{aligned}
&+ 5{\cdot}90 \sin\{[n-24n''+20n'''] t + \epsilon - 24\epsilon'' + 20\epsilon''' + 3\varpi'''\} \\
&- 2{\cdot}33 \sin\{[n-24n''+20n'''] t + \epsilon - 24\epsilon'' + 20\epsilon''' + \varpi'' + 2\varpi'''\} \\
&+ 0{\cdot}10 \sin\{[n-24n''+20n'''] t + \epsilon - 24\epsilon'' + 20\epsilon''' + 2\varpi'' + \varpi'''\} \\
&+ 0{\cdot}04 \sin\{[n-24n''+20n'''] t + \epsilon - 24\epsilon'' + 20\epsilon''' + 3\varpi''\}
\end{aligned}$$

or

$$\delta n = + 7''{\cdot}55 \sin\{[n-24n''+20n'''] t + \epsilon - 24\epsilon'' + 20\epsilon''' + 284^\circ\ 14'\}.$$

Writing it as a term increasing with the time, then

$$\delta n = \sin\{53''{\cdot}2\ (T - 1807{\cdot}6)\},$$

where T is the year for which the value of the inequality is required. The inequality has therefore a period of 406·0 years.

In the *Monthly Notices* for November 1873, there is a paper by Sir George Airy bearing on the lunar theory. The Astronomer Royal there remarks:—"It appears, therefore, that after applying the best correction that we are able to give to the assumed mean longitude and mean motion, there still remains a series of unexplained discordances, varying from $-6''{\cdot}79$ to $+4''{\cdot}93$. And each of the discordances here exhibited is based on about 440 observations; these observations are all reduced by the same system of clock stars and other elements (Bessel's in the *Tabulæ Regiomontanæ*), and the tabular places are all computed by the same theory (Plana's with Damoiseau's epochs).

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"I conceive it to be totally impossible that a complete and correct theory can leave such large and systematic discordances. And I express my opinion that there is still some serious defect in the lunar theory." (*Monthly Notices*, xxxiv., p. 5.)

The Astronomer Royal has tabulated these residual errors for groups of about $4\frac{1}{2}$ years, between 1750 and 1851. It was thus easy to see how far the newly discovered term reduces or augments these residual discordances. It was therefore gratifying to find that for the period between 1780 and 1851 it completely accounts for them. For this period the mean of the outstanding positive errors is reduced to about $0''.7$, and the mean of the outstanding negative errors is reduced to about $0''.6$. It does not account so well for the residual discordances between 1765 and 1780, and fails to account for the errors in the period 1750–1765. I have, however, discovered a minor term which will partially account for the errors in these earlier observations between 1750 and 1770.

From the nature of the new term it is obvious that the value which has been deduced for it in the present paper is at best merely provisional. It will require most careful revision, and the higher powers of the disturbing forces must be taken into account. This will involve some labour, because, owing to the great eccentricity of the orbit of *Mars*, the convergency of the series which it involves is slow. It is probable that when the complete value of the term is ascertained, it will be found to be slightly smaller than that depending on the first power of the disturbing forces. Under these conditions it would still better represent the observations. The existence of this term is so strongly supported by observation, from the manner in which it removes the discordances between theory and observation detected by Sir G. Airy, that I thought it better to publish it in its present form, rather than defer this for the considerable period which may elapse before I am able to verify these results.

I take this opportunity to announce that I have discovered other terms in the mean motion of the Moon, arising from the direct action of the planets, and having sensible coefficients. I hope to be able to bring these before the Society in a short period. One of these, having the comparatively short period of 60 years, appears to be strongly indicated by observation.

London, Nov. 2, 1877.

On Three Small Inequalities in the Mean Motion of the Earth, and an Inequality in the Mean Motion of Mars. By E. Neison, Esq.

Whilst engaged on some investigations connected with the perturbations in the motion of the Moon produced by the action of the planets, I came across several terms of long period in the

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mean motion of the Earth which it appeared might possibly have a sensible value. These terms depend on the second power of the disturbing force. Hitherto the only two terms of long period of this character which have been made known are the two terms discovered by Leverrier, and embodied in his solar tables. (*Annales de l'Observatoire*, tome iv.) These terms are, adopting Leverrier's notation,

$$6''.410 \sin\{[4n'' + 3n^{IV} - 8n'''] t + A\}, \qquad 0''.292 \sin\{[7n'' - 3n' - 4n'''] t + A_1\},$$

the first having a period of 1,800 years and the second a period of 300 years.

The first new term depends on three times the mean motion of the Earth, plus three times the mean motion of *Jupiter*, minus twice the mean motion of *Venus*. In fact $[3n'' - 2n' + 3n^{IV}] = 2419''$ only, so that the term has a period of 530 years, which might give it a sensible value, although multiplied by the fourth power of the eccentricity of *Jupiter*. On computing its value, it was found, however, that all the principal terms destroy each other, so that the largest coefficient does not amount to $0''.01$. There is another term of still longer period, namely that depending on the relation $[6n'' - 5n' + 4n'''] = 1140''$, but this also will be probably entirely insensible.

The argument of the second term depends on the mean motion of the Earth, plus twice the mean motion of *Saturn*, minus twice the mean motion of *Mars*. As $[n'' + 2n^{V} - 2n'''] = 5868''$, and the term is of the first order of the eccentricities, it might have yielded a term with a sensible coefficient. On calculating the different terms, the only sensible one was found to be

$$+0''.0178 \sin\{(n'' - 2n''' + 2n^{V}) t + \epsilon'' - 2\epsilon''' + 2\epsilon^{V} + \varpi'''\}.$$

This term, although larger than many retained by Leverrier in his tables of the Sun, is insignificant. A similar but larger term must exist in the mean motion of *Mars*, and by calculating its value it was found to be

$$-0''.1275 \sin\{(2n''' - n'' - 2n^{V}) t + 2\epsilon''' - \epsilon'' - 2\epsilon^{V} + \varpi''\}.$$

It has a period of 220·9 years, and, being capable of altering the geocentric longitude of *Mars* by nearly half a second of arc, deserves some attention.

The third term depends on ten times the mean motion of the Earth, minus six times the mean motion of *Venus*, and three times the mean motion of *Jupiter*, and is therefore analogous to the very important term discovered by Leverrier, and depending on the mean motion of the Earth, *Mars*, and *Jupiter*. Inasmuch as $[10n'' - 6n' - 3n^{IV}] = -7846''$, the term has a period of 165 years. The principal terms mutually destroy each other, so that the coefficient is not one-tenth of what might have been expected.

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The main outstanding terms are the following:—

$$+\{0\cdot0811-0\cdot1578 = -0\cdot0767\}\sin\{(10n''-6n'-3n^{IV})t+10\epsilon''-6\epsilon'-3\epsilon^{IV}-\varpi''\}$$

$$-\{0\cdot0273-0\cdot0384 = -0\cdot0111\}\sin\{(10n''-6n'-3n^{IV})t+10\epsilon''-6\epsilon'-3\epsilon^{IV}-\varpi'\}$$

$$+\{0\cdot0080-0\cdot0171 = -0\cdot0091\}\sin\{(10n''-6n'-3n^{IV})t+10\epsilon''-6\epsilon'-3\epsilon^{IV}+\varpi^{IV}-2\varpi''\}.$$

$$+\{-0\cdot0057\}\sin\{(10n''-6n'-3n^{IV})t+10\epsilon''-6\epsilon'-3\epsilon^{IV}+\varpi^{IV}-\varpi''-\varpi'\}.$$

There is a peculiarity about this term which I am in hopes of examining. According to Leverrier the mean motion of *Neptune* is equal to $7872''\cdot7$. Therefore the term just considered in the mean motion of the Earth will give rise to another term of the form

$$\{10n''-6n'-3n^{IV}-n^{VII}\}, = -26''.$$

This will be a term independent of the eccentricities, and of the third power of the disturbing forces, and with an immensely long period of 50,000 years, or nearly twice the period of the revolution of the Earth's perihelion. It is therefore very probable that this term possesses a comparatively considerable coefficient. It is true, from its immense length of period, the term is without importance as far as tables of the position of the Earth is concerned, but it possesses a scientific interest. A slight change in the value of the mean motion of *Neptune* would convert this term into a secular term, by making

$$\{[10n''+2\varpi''-6n'-3n^{IV}-n^{VII}]\} = 0.$$

My principal reason for communicating these results is to let it be known that they have been examined. Hansen calculated many terms of this nature in the theory of the Earth, but he has nowhere stated what terms he so examined. This is to be regretted, as it leaves this fruitless and laborious work to be performed again by anyone traversing the same ground. Similarly, had Hansen and Delaunay published a list of the terms produced by the planets in the motion of the Moon which they had examined, it would have materially lightened the labour of anyone endeavouring to discover the missing terms of long period in the motion of the Moon.

Oct. 29, 1877.

The Satellite of Neptune. By J. W. Ward, Belfast.

The following observations were all made without any knowledge of the position angles or distances by Ephemeris, which was *afterwards* consulted. On the whole I do not think that this satellite is more difficult than the two outer satellites of *Uranus* as observed in the spring of 1876. The points were glimpsed by averted vision.

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Telescope 4·3 in., Wray, power 175.

Date. 1877.	Local M.T. of Obs.	Estimated P. Angle.	P. Angle Ephem. at 11 hours G.M.T.	Dist. Ephem. at 11 hours G.M.T.	Estimated Dist.	
		°	°	″	″	
		±			±	
Oct. 16	12½ H.	75	86	7·45	10	Moonlight.
„ 17	11 „	45	44	16·7	15	do.
„ 27	12 „	170	194	10·5	10	Another point: P. 300°; D. 15″ ±
„ 29	12 „	40	40·2	17·0	15	
„ 30	11 „	350	10·6	9·8	10	
Nov. 6	11¼ „	240	241·9	11·4	10	

Coggia's Comet, 1877.

(*Communicated by Prof. Pritchard, University Observatory, Oxford.*)

From an observation at Marseilles, on September 14, and two others at Oxford on October 6 and 31, the following elements were computed :—

T	Sept. 11·40830 G.M.T.		
	°	′	″
π	107	37	41·2
Ω	250	58	48·9
i	77	42	26·9
log *q*	0·1977141		

Motion retrograde.

The middle observation is represented within the errors—

		″
C−O	*dl* cos *b*	+7
	db	−3

Ephemeris of Coggia's Comet for Greenwich Mean Midnight.

	R.A. h	m	s	N.P.D. °	′	log Δ
Nov. 12	6	5	45	73	10·8	9·9634
16	5	46	0	77	41·8	9·9562
20	5	25	56	82	20·6	9·9562
24	5	6	14	86	54·7	9·9636
28	4	47	23	91	12·1	9·9778
Dec. 2	4	29	49	95	3·9	9·9978
6	4	13	49	98	25·3	0·0220

The computations were made by Mr. W. Plummer.

Oxford, Nov. 9, 1877.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. *December* 14, 1877. No. 2.

WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

C. J. Lambert, Esq., 1 Crosby Square, E.C., and
Arthur Schuster, Esq., Ph.D., St. John's College, Cambridge;

were balloted for and duly elected Fellows of the Society.

Mr. D. Gill's Expedition to Ascension. Report, November 11, 1877.

We returned from Green Mountain on October 16 and I resumed observations the same evening.

During the past month the weather has been most unfavourable. Had the opposition of *Mars* occurred in October the result would have been complete failure: only one series of morning observations would have been possible, though there were a good many clear evenings. I have chiefly been occupied with the triangulation of the *Mars* stars of comparison; indeed, when they were conveniently situated for observation I have devoted myself exclusively to this work. Three good nights' work will now finish it, and I hope to send a complete copy of the observations by next mail.

Some observations have been made by daylight of the distance and position-angle of the stars α_1 α_2 *Centauri*. The results show a very small increase in the position-angle, and a marked diminution of the distance (viz. from about $2''{\cdot}15$ to $1''{\cdot}75$) as compared with the results previously communicated. It is probable therefore that very soon the distance will be too small for measurement with so small an aperture (4 inches); as it is, observations are only possible in exceptionally favourable circumstances. I delay sending more detailed results till the series is complete.

In the Melbourne Catalogue there is a star, ϵ *Indi*, having the very remarkable annual proper motion of $4''{\cdot}2$.

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As it happens, this star is favourably situated for observation when the *Mars* stars are in the zenith, and when they, for "break-neck" reasons, are almost unobservable. I have therefore measured the distance and position-angle of ε *Indi* relative to five neighbouring surrounding stars, and hope this may serve as the foundation at some future day of a determination of its parallax and proper motion. The distance of *Saturn* from *Mars* was measured on five days, symmetrically disposed with respect to the remarkable conjunction of November 3. The result may afford some interesting data as to the accuracy attainable by means of the heliometer in such measures, and give a determination of the relative positions of these planets which may be of value for future reference.

The proposed observations of *Melpomene* should begin on the 12th inst. (to-morrow), and I trust the weather will suddenly become more favourable.

Mars Bay, Ascension Island,
November 11, 1877.

Remarks accompanying Drawing of Mars. By Geo. D. Hirst, Esq.

As *Mars* during this opposition is not so favourably placed for observation in your latitude as he is here, I venture to forward the enclosed sketch in the hope that it may prove of interest. The drawing has been carefully executed from a fine 10¼ in. reflector, and represents very nearly the appearance of the planet at the present time.

Until lately the markings on *Mars* have been very indistinct, or almost invisible. Even on nights of the finest definition they have been too faint to attempt any effort at delineation, but within the last few days whatever it is that has interfered with our view, either in our own atmosphere or that of the planet, has removed, and the features I have endeavoured to depict are very distinct.

379 *George Street, Sydney, N. S. Wales,*
August 24, 1877.

The Physical Condition of Mars. By John Brett, Esq.

The following remarks are founded on a series of telescopic observations of the planet from August 2 to October 8, 1877, made with a 9-inch (With-Browning) reflector at the southern extremity of England near the Lizard signal station. The conditions were favourable, and no good opportunities of observation

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were neglected; but the results I have to lay before the Society are in one sense extremely unsatisfactory, inasmuch as their tendency is to disturb, if not overthrow, conclusions which have hitherto met with general acceptance. The instrument employed, although of very moderate aperture, is good, and in vindication of its quality it will be sufficient to record that I have had a satisfactory view of the outer satellite of *Mars* with it (on the 15th of September) and, at an earlier date, of the satellite of *Neptune*.

As regards the disk of the planet *Mars*, I must call it emphatically a bad telescopic object, for, under the same conditions of altitude and steadiness, its image will not bear comparison for a moment with *Jupiter* in respect either of quantity or beauty of delicate detail; whilst *Saturn*, as compared with it, is immeasurably superior.

All the modern drawings of *Mars* exhibit well-defined half-tints pervading the so-called seas. I cannot give any account of any of these half-tints. There are certain parts of the dark spaces considerably darker than others, notably so the principal round spot in the equatoreal region, and the conspicuous peak next preceding it. But the fact of these markings passing nearly over the centre of the disk, where of course they are seen through a less depth of atmosphere, is sufficient to account for their superior intensity, for the round spot at its first appearance, some two or three seconds within the limb, is not above the average darkness of the markings.

This circumstance alone I consider sufficient to establish the fact that the planet's atmosphere possesses such a degree of opacity as to prevent the telescope doing itself justice on the details, and the total disappearance of these details two or three seconds within the limb of course removes all doubt about their being below the surface. The opacity of the atmosphere is further and most conclusively shown, as I think, by the great brightness of the disk at the limb. The contrast with *Jupiter* in this respect is most interesting.

It is not known that *Jupiter* has any solid nucleus, nor any atmosphere, strictly so called. We only know that he is semi-transparent and limpid to an enormous depth; and his brightness fades away sensibly towards the limb. *Mars*, on the contrary, is believed to be a solid body (his general topography being permanent), with a true atmosphere, and his brightness conspicuously increases towards the limb. The theory that the redness of the so-called land was owing to absorption in the planet's atmosphere is obviously untenable, for the limb, where the atmosphere alone is visible, is nearly white, whilst the central part of the disk is of a glowing red, very like any ordinary solid body heated to a dull red heat. That it is really red hot would of course be inconsistent with the received theory of the snows and seas, but there is reason to believe both these must be given up.

There is one salient fact about *Mars* which I fear cannot be

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gainsaid, and if that is so, the hypothesis that his surface is habitable must be abandoned. There are no clouds.*

The past autumn has been singularly favourable for observation; I remember none equal to it.

Between the 2nd of August and the 8th of October there has been good definition on eighteen nights. There have been many more observing nights, but I have only counted those when it was worth while to draw. On no one occasion during this series of fine nights has there been a moment's doubt about the identification of the well-known features of the so-called seas. No one of them has been masked or disfigured beyond recognition. Now, supposing there to be land and sea, the evaporation and consequent cooling should be unequal in different regions, and currents must be generated, and therefore clouds wafted hither and thither; and the seas would thereby be sometimes hidden.

Terrestrial clouds are as opaque as anything in nature. The locality of the midday Sun cannot even be suspected when masked by an ordinary cloud, much less could the difference between land and sea be perceived.

That a whole hemisphere should be entirely exempt from clouds for more than two months continuously is, I submit, fatal to the seas.

No man will say that the atmosphere may not be dense enough to sustain clouds, for its density is obvious to the eye; besides, the so-called snows have to be accounted for. Between vapour and snow, the intermediate condition of cloud, or chilled vapour, is inevitable.

If any man says the body of *Mars* is so hot that the vapour will not condense in clouds, I have no objection to offer; but boiling seas are hardly satisfactory.

The white polar patch has always been regarded as snow; but there are one or two things to be said which are unfavourable to this hypothesis, besides the want of clouds to deposit it. In the first place the south polar patch now in view is surrounded with a dark stain, and this darkness is on one side continuous with the so-called sea, and of equal darkness with it. There is no break nor barrier in the straits which join the polar dark patch to the equatoreal dark patch. Therefore, if there be snow, it is lying on the sea, or on a polar island.

Beer and Mädler are said to have thought there was a marsh at the borders of the melting snow to account for this dark belt, but most of us would be unable to distinguish a marsh from a meadow when about a mile off. But the worst thing I have to say against the snow is that it lies far above the level of the solid body of the planet. Everybody knows that it appears to project beyond the limb in profile, but the irradiation of its greater brightness has been supposed to explain that away.

The *dark* details of the solid surface disappear long before

* A hypothetical exception is mentioned later.

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they reach the limb, but the *white* detail sticks out beyond it; so that irradiation has a good deal to answer for.

Now I assert that the white patch is not continuous with the surface of the solid, but stands above it; and the evidence I adduce that such is the case consists simply in the shadow of the white patch, which could recently be seen to the eastward after the planet had passed its opposition. My first memorandum of this appearance is dated September 28 at 9^h, definition being on that occasion the very finest I have had this autumn.*

The first occasion on which the gibbosity of the disk was noticed was the 5th of October. It was then conspicuous.

The only hypothesis I offer respecting the white polar patch is that it consists of cloud: the neighbourhood of the pole being supposed to be the only region cool enough to condense the vapour.

There is no doubt that cloud would be bright enough, compact enough, and definite enough to supply all the observed peculiarities.

The Earth can show nothing whiter or more sharply defined than its cumulous clouds; and in *Mars*, if the temperature is so great as to dissipate them everywhere else, it is presumable that they would, even at the pole, form themselves only at the outer regions of the atmosphere.

This hypothesis accounts both for the apparent projection and the visible shadow.

The sum of my autumn's work upon *Mars*, therefore, is not only disappointing but grievous. The snows I am sure must be given up, and I sadly fear the seas must go too; and the one little companion globe in the whole solar system on which we could depend has its character taken away.

P.S. Since the 9th of October a patch of light has been noticed to the N.W. of the polar patch, of a diffused character, probably caused by specular reflexion from the outer surface of the envelope, since it is believed not to be affected by the rotation. This requires further observation.

November 9, 1877.

Notes on Mars, 1877. By H. Pratt, Esq.

Opportunities of observing the planet have not been numerous, for the weather here has often been indifferent. Yet on several occasions when definition has been really fine some of the markings have been very definite, and although the majority presented softened outlines, yet their complexity could only be realised by close and patient attention. On such occasions the number and variety of curved forms has been constantly remarked.

* It cannot be seen with ordinary definition.

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In moments of the finest definition the markings have exhibited a *stippled* rather than a streaked character, and glimpses were obtained of a structure so complicated and delicate that the pencil cannot reproduce it, for the visual perception of it was too transient to be retained. Frequently what at first sight appeared as a broad hazy streak has been, by patient watching for the best moments, resolved into several separate masses of shading enclosing lighter portions full of very delicate markings. The spaces enclosed by the markings were sometimes of the general reddish tint, and occasionally almost white.

From the discrepancies that will occur between the most careful drawings of the same side of the globe made on different nights, and from the difficulty of the identification of the forms shown by the published drawings of previous observers, it has been difficult to believe that the perfect and *complete* form of any particular marking is ever seen on any one occasion. Hence the multiplication of careful drawings appears the only way to arrive at a fair knowledge of the features of the planet.

The persistence with which many well-known markings recur again and again during many years, as they have recurred from night to night during the present season, surely proves their connection with the globe itself rather than with its atmospheric envelope. And yet the difference in the details of any one form as seen with the same instrument, by the same observer, on different occasions, while our own atmosphere has been equally clear, as tested by reference to *Saturn*, surely points to variations in the opacity of the aerial envelope of *Mars* as the cause. Indeed, the idea of *local obscurations* as from a cloudy condition of the planet's atmosphere has seemed sufficient to account for many such discrepancies, though why certain forms should be visible at one time, and either *obliterated* or *greatly modified* at another time, it is not easy to decide. Drawings II. and VI. illustrate these difficulties.

The *exaggeration of the polar cap* by irradiation, which causes it at times apparently to extend beyond the planet's limb, has been observed a few times, but only on nights which, for other reasons, were deemed insufficiently good for any delicate observation. On the finest nights the polar cap has always appeared precisely as if it formed a portion of the actual surface of the globe.

On every occasion of really good definition the markings were most easily made out on the following limb, and often very closely approached it, as was remarked by Capt. Noble and others at the November meeting.

Although generally the atmospheric envelope of *Mars* has appeared clear by allowing the markings to be well seen, yet frequently the great difficulty of detecting any markings at all over large regions where markings undoubtedly do exist has seemed to indicate the existence of a partially opaque condition, although no well-defined cloud-masses like those observable on *Jupiter* were ever detected. That this effect was not caused by our own atmosphere has been proved by the superior definition

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of *Saturn* at a lower altitude. This has occurred on different occasions, and is instanced on November 14, when, in the S.W. quadrant, the region bounded by the curved shading shown on Drawing VI. was of that character, and the whole region was whitish, the otherwise prevailing red tint being absent.

On every observing night this season the prevailing tint of the red portions of the planet has been much paler than during recent oppositions.

The general colour of the markings has appeared of a neutral gray, that of the most intensely marked portions inclining to bluish. Not the slightest trace of green has been detected.

The intense whiteness of the polar caps during August has gradually declined in purity of tint since then, becoming more nearly the same as the limb.

On all sides towards the limb the redness has always paled rapidly almost to white.

The six drawings accompanying these notes are selected from a series, and refer to nights when the best definition prevailed.

The longitudes appended are derived from Mr. Marth's Tables.

The instrument a Newtonian Equatoreal, speculum by With, of 8·15 in. aperture, diagonal prism of fine quality, power 400 single achromatic.

The Approximate Fraction Elements of Solar System.

By S. M. Drach, Esq.

In the weekly *English Mechanic* for June 16 and July 7, 1871, a writer signing himself G. F. H. gave remarkably close and simple approaches to the orbital mean distances and eccentricities, which I think ought to be specially recorded in the *Monthly Notices*. I have corrected the faulty 487 of *Saturn's* denominator, and extracted the factors:

Mercury	·3870981 − ·0000013 =	$\frac{12}{31}$;		
Venus	·7233316 +	17 = $\frac{217}{300}$,	or	$\frac{7.31}{10.30}$;
Earth	1 −	00 = 1;		
Mars	1·5236923 −	15 = $\frac{611}{401}$,	or	$\frac{13.47}{401}$;
Jupiter	5·2027760 +	18 = $\frac{1873}{360}$,	or	$\frac{11.31.131}{6.27.53}$ true;
Saturn	9·5387861 −	30 = $\frac{4550}{477}$,	or	$\frac{5.7.13}{9.53}$;
Uranus	19·1823900 −	01 = $\frac{4051}{159}$,	or	$\frac{4051}{3.53}$;
Neptune	30·0566000 +	40 = $\frac{1593}{53}$,	or	$\frac{27.59}{53}$.

Remark the 1, 3, 9, 27 times 53.

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For eccentricities.

Mercury	·2054925 − ·0000001 =	$\frac{217}{1056}$, or	$\frac{7.31}{32.33}$;
Venus	·0068722 + 07 =	$\frac{2}{291}$, or	$\frac{2}{3.97}$;
Earth	·0167918 − 08 =	$\frac{9}{536}$, or	$\frac{9}{8.67}$;
Mars	·0931125 − 03 =	$\frac{73}{784}$, or	$\frac{73}{28.28}$;
Jupiter	·0481626 − 04 =	$\frac{38}{789}$, or	$\frac{2.19}{3.263}$;
Saturn	·0561502 − 05 =	$\frac{21}{374}$, or	$\frac{21}{17.22}$;
Uranus	·0466683 − 17 =	$\frac{7}{150}$, or	$\frac{7}{10.15}$;
Neptune	·0084962 − 00 =	$\frac{10}{1177}$, or	$\frac{10}{11.107}$.

Nov. 1877.

On the Synodisms of Saturn's Satellites. By S. M. Drach, Esq.

In Mr. Christie's journal, *The Observatory* (No. 7, for last month), Mr. D. Kirkwood deduced from Lovnics' *Astronomy* (by adding $0^s.62$ to period of *Mimas*) this interesting formula of the four innermost moons:

$$5(n_1 - n_3) + (n_3 - n_2) + 4(n_4 - n_2) = 0.$$

In this form I think it may lead to a theory akin to that arising from the relation to *Jupiter's* lunar $n_1 - 3n_3 + 2n_2 = 0$ between the mean motions of *Jupiter's* satellites. Recommended to try later elements for all, I have selected Guillemin's *Heavens* (English translation, 1876). Chambers' *Descriptive Astronomy* only gives the *minutes*; the columns of "days and decimals" could have been better replaced by the *seconds* of time. (*Saturn's* rotation, $10^h\ 29^m\ 17^s$).

		Period. d	h	m	s	In *Sat.* days.	Rev. in *Sat.* year.
I.	M.	0	22	37	23	2·157305	11414·08
II.	E.	1	08	53	07	3·135499	7852·185
III.	Te.	1	21	18	26	4·319888	5699·331
IV.	D.	2	17	41	09	6·262918	3931·155
V.	R.	4	12	25	11	10·33745	2381·678
VI.	Ti.	15	22	41	25	36·48820	675·2189
VII.	H.	22	07	07	41	51·02262	482·5411
VIII.	J.	79	07	54	40	181·5310	135·6271
	⊙		10759^d·22			24625·1	unity

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Synodisms in Chronological Order.

Syn.	Terrestrial. d	h	m	s	*S.* days.	Syn.	Terrestrial. d	h	m	s	*S.* days.
M.J.	0	22	13	42	2·18	E.R.	3	02	48	41	7·13
M.H.	0	23	37	19	2·23	Te.R.	3	05	49	57	7·42
M.Ti.	1	00	02	40	2·30	D.Ti.	3	07	17	47	7·56
M.R.	1	04	35	18	2·72	E.D.	3	10	54	26	7·91
E.J.	1	09	47	46	3·19	R.J.	4	18	58	17	10·96
M.D.	1	10	30	31	3·29	E.Te.	4	23	56	39	11·44
E.H	1	11	02	19	3·34	R.H.	5	15	58	21	12·97
E.Ti.	1	11	58	37	3·43	Te.D.	6	02	01	37	13·92
M.Te.	1	21	11	07	4·25	R.Ti.	6	10	36	42	14·42
Te.J.	1	22	59	43	4·42	D.R.	6	22	39	03	15·89
Te.H.	2	01	29	53	4·71	Ti.J.	19	22	57	41	45·68
Te.Ti.	2	03	23	29	4·87	H.J.	31	00	20	13	70·88
D.J.	2	20	01	59	6·49	Ti.H.	55	23	25	22	128·12
M.R.	3	00	26	43	6·91						
D.H.	3	02	31	58	7·11						

⊙ 24625·1 *Sat.* days.

Fried. Weis published in 1860 *Gesetze der Satellitenbildung* (Perthes, Gotha, 8vo.). In the Library of the Royal Geographical Society.

Nov. 16, 1877.

On a New Astrophotometrical Method. By Prof. Ch. V. Zenger.

It occurred to me to measure the intensity of the light of planetary disks by the time they take appearing or disappearing in twilight, and I first tried it in April, when *Jupiter* was a morning star.

I was surprised at the beautiful regularity and the order in which the details of the planetary disk vanished, and the accordance of the determined intensity of light as well of the four satellites as of the details on the planetary disk.

It is obvious that the time of appearance and disappearance will be different according to the intrinsic luminosity of celestial objects, and that the object will vanish as soon as the heavenly background acquires by reflected light the same intensity as the observed star.

These observations are so easy, and the vanishing of the light is so obvious, that the method will give; even with untrained yet sufficiently sensitive eyes, good results, and will enable every possessor of a telescope from 2 inches aperture upwards to do with it useful work.

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Most surprising is the result obtained by this photometrical method in the case of *Jupiter*, enabling us to judge of the relative brightness of the limb, of the planetary centre, of the different parts of the belts and polar zones, and lastly, of the brightness of the Jovian satellites.

It is a known fact, by the measurements of actinic and optic intensity of reflected sunlight from the atmosphere made by Prof. Bunsen and Roscoe, that both give parallel curves of increase and decrease at the same place of observation, and that the intensity is represented by the following formula:—

$$I = a + b \cos z - c \cos^2 z, \qquad (1)$$

I denoting intensity of reflected sunlight, a, b, c constants, and z the zenith distance of the Sun.

It is obvious that in our case z is very nearly 90°, and the term $c \cos^2 z$ very small, and we thus get approximately

$$I = a + b \cos z,$$

or

$$i = \frac{I}{a} = 1 + \frac{b}{a} \cos z = 1 + A \cos z, \qquad (2)$$

A denoting a new constant, that may be determined by supposing the intensity to become zero when the Sun is 18° below the horizon. We obtain

$$i = 1 + A \cos 108° = 1 - A \sin 18° = 0,$$

and therefore

$$A = 3{\cdot}2361 \qquad (3)$$

By a known formula we obtain

$$\cos z = \sin \delta \sin \phi + \cos \delta \cos \phi \cos t = \cos \phi \cos \delta (\cos t + \tan \phi \tan \delta),$$

or

$$I = 1 + 3{\cdot}2361 \cos \phi \cos \delta (\cos t + \tan \phi \tan \delta). \qquad (4)$$

Supposing the Sun's declination to be $\delta = 0$, and $t = 90° = 6^h$, we obtain, in equinoctial time, the Sun being in the horizon, $I = 1$; the unit for photometrical measurement.

Tab. 1.—It is from the above formula (4) I calculated a table to give, by interpolation, the brightness of twilight for Sun's declination $= +20°$ to $-20°$ and $t = 4^h$ to $t' = 8^h$.

Tab. 2.—The second table gives the observations of *Jupiter* April 1877 to end of October, containing time of appearance and disappearance of Jovian details and satellites with a 4-inch equatoreal, power 56 to get a large field.

Tab. 3.—It is obvious that the condition of the atmosphere will affect the absolute time of disappearance and appearance,

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but not the relative time of it; I therefore computed the brightness relative to the third satellite, which gave the least variations of brightness of all in the Jovian system. The results are given in the third table.

The result is that the order of Jovian details, as shown by the means of observed brightness, will be as follows:—

First in brightness was observed the northern limb of the planet's disk, near the equator, then follow the middle part of the northern equatoreal belt, the northern polar zone, the southern belt (middle part of it), the southern polar zone; next to it comes the third satellite, its brightness differing only 5 per cent. from the brightness of the planetary disk near the southern pole, the second and first satellites both very nearly of the same brightness, yet sometimes interchanging in brightness, and finally the fourth satellite, the weakest in brightness, yet very variable, attaining sometimes nearly the brightness of the second satellite, yet decreasing to nearly one-half of the brightness of the third satellite.

The means, computed as shown above, are:

		Satellites.	
Limb of planetary disk	1·555		
Equatoreal belt, N.	1·130	Third	1·000
Polar zone, N.	1·124	Second	0·970
Equatoreal belt, S.	1·110	First	0·961
Polar zone, S.	1·091	Fourth	0·820

A period of change of brightness seems to be indicated with the fourth and second satellites, the period being as a mean for the former 16·6 days, for the second nearly two days, both in accordance with the time of revolution. The small number of observations, and, still more, the small amount of change in the first and third satellites did not allow of deducing periods of change of brightness for these. As *Venus* becomes now visible for the smallest telescopes during daylight, it would be interesting to apply this method of observation to the planetary details, so as to determine the influence of reflecting power, apparent breadth of illuminated disk, and distance, the time of appearance of light, if any there be, on the dark part of disk, etc. The method being so plain, and requiring no apparatus for photometric measurement, I supposed it of interest to any possessor of a telescope from 2 inches upwards.

Prague, Oct. 17, 1877.

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On the Relative Brightness of the Details of Jupiter's Disk and Satellites.

TABLE I.

Hour Angle.			*Intensity of Twilight.* ⊙'s Declination.									*Cosines of Zenith Distances.*								
h	m	°	−20°	Δ1°	−10°	Δ1°	0°	Δ1°	+10°	Δ1°	+20°	−20°	Δ1°	−10°	Δ1°	0°	Δ1°	+10°	Δ1°	+20°
4	0	60	0	1·124		1·570		2·000		2·398		2·757	+0·040		−0·183		+0·321		0·449		+0·564
					0·045		0·043		0·040		0·036			0·014		0·014		0·013		0·011	
	40	70	0	0·850		1·258		1·680		2·087		2·458	−0·056		−0·083		0·219		0·349		0·468
					0·041		0·042		0·041		0·037			0·002		0·014		0·013		0·011	
5	20	80	0	0·504		0·926		1·345		1·753		2·141	−0·159		−0·024		0·111		0·242		0·366
					0·042		0·042		0·041		0·041			0·014		0·014		0·013		0·012	
6	0	90	0	0·183		0·586		1·000		1·414		1·815	−0·262		−0·133		0·000		0·133		0·262
					0·040		0·041		0·041		0·040			0·013		0·013		0·013		0·013	
	40	100	0	...		0·246		0·655		1·074		1·796	−0·365		−0·242		−0·111		0·024		0·159
							0·041		0·042		0·042			0·012		0·012		0·013		0·013	
7	20	110	0	...		...		0·318		0·745		1·175	−0·468		−0·349		−0·219		−0·082		0·056
									0·042		0·043			0·012		0·012		0·014		0·014	
	0	120	0	...		...		0·000		0·430		0·879	−0·564		−0·449		−0·321		−0·183		0·039
									0·043		0·045			0·012		0·012		0·014		0·014	

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Table II.

Date.	The Satellites. (4)	(3)	(2)	(1)	Equatoreal Belts. South Limb.	South Middle.	North Limb.	North Middle.	Polar Zones. South.	North.	Limb of Disk.	Two Fixed Stars near Jupiter's Sat. (4).
	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m	h m
April 7	17 8·0	17 17·5	17 11·0	17 16·0	17 42·0	17 44·0	17 46·0	17 59·0	18 0·75	18 15·0	18 25·5	...
June 12	15 16·0	15 26·1	15 24·1	15 21·0	...	...	...	...	...	...	...	...
„ 14	15 20·8	15 30·1	15 23·75	15 26·7	...	...	...	...	...	...	...	...
Sept. 11	6 39·0	6 26·0	6 28·0	6 36·0	...	6 15·0	...	6 25·0	6 19·0	6 25·5	...	...
„ 28	6 30·0	5 47·0	6 0·25	5 48·25	...	5 42·0	...	5 42·0	5 54·0	5 45·0	...	6 43·0 F_1
„ 30	6 35·0	5 54·25	5 56·5	6 1·0	...	5 40·0	...	5 40·0	5 48·0	5 45·0	...	6 37·0 F_1
Oct. 6	5 55·5	5 42·25	5 44·5	5 43·5	...	5 28·0	...	5 28·0	5 32·5	5 31·0	...	6 15·0 F_2
„ 7	5 46·25	5 36·5	...	5 39·0	...	5 26·0	...	5 26·0	...	5 28·5	...	6 14·0 F_2
„ 14	5 58·5	5 28·0	...	5 34·5	...	5 17·0	...	5 15·0	5 27·0	5 21·0	...	6 2·0 F_2
„ 21	5 15·0	5 37·5	5 8·25	5 6·25	...	4 50·0	...	4 52·5	...	4 56·0	4 45	...
„ 27	5 5·3	4 56·2	4 57·4	4 59·0	...	5 53·5	...	4 53·0	5 4·5	4 50·3	...	...

Table III.

Equinoctial Twilight—1·000 at 6h.

April 7	0·805	0·891	0·800	0·877	1·102	1·120	1·138	1·206	1·280	1·408	1·502	...
June 12	0·751	0·870	0·848	0·811		...		...	...	...	...	...
„ 14	0·753	0·806	0·769	0·787		...		...	...	...	...	...
Sept. 11	0·816	0·933	0·925	0·846		1·032		1·032	0·938	1·014	...	...
„ 28	0·588	0·934	0·926	0·828		0·974		0·974	0·878	0·950	...	0·486 F_1
„ 30	0·538	0·900	0·881	0·834		1·052		1·052	0·977	1·005	...	0·481 F_1

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TABLE III.—*continued.*

Date.	The Satellites. (4)	(3)	(2)	(1)	Equatoreal Belts. South Limb.	South Middle.	North Limb.	North Middle.	Polar Zones. South.	North.	Limb of Disk.	Two Fixed Stars near *Jupiter's* Sat. (4).
Oct. 6	0·769	0·835	0·825	0·817		0·949		0·949	0·913	0·925	...	0·573 F_2
„ 7	0·864	1·014	...	1·004		1·056		1·056	...	1·046	...	0·632 F_2
„ 14	0·884	0·937	...	0·929		1·027		1·043	1·027	1·043	...	0·654 F_2
„ 21	0·935	1·055	1·014	1·032		1·149		1·127	...	1·127	1·059	...
„ 27	0·941	1·023	0·996	0·979		1·027		1·033	1·029	1·033	...	...
Means:	0·785	0·914	0·880	0·791		1·043		1·053	0·962	1·063	1·280	0·484
Sat. (3) = 1·000:	0·855	1·000	0·939	0·862		1·141		1·152	1·053	1·163	1·401	0·620
April 7	0·91	1·000	0·93	0·99		1·25		1·32	1·43	1·43	1·86	...
June 12	0·85	1·000	0·97	0·93		...		...	...	...	...	...
„ 14	0·93	1·000	0·95	0·98		...		...	...	...	...	...
Sept. 11	0·88	1·000	0·99	0·91		1·10		1·10	1·00	1·08	...	...
„ 28	0·62	1·000	0·99	0·94		1·03		1·03	0·94	1·01	...	0·52
„ 30	0·60	1·000	0·97	0·92		1·17		1·17	1·08	1·16	...	0·53
Oct. 6	0·91	1·000	0·99	0·98		1·14		1·14	1·02	1·04	...	0·67
„ 7	0·85	1·000	...	0·99		1·04		1·04	...	1·03	...	0·62
„ 8	0·95	1·000	...	0·99		1·10		1·11	1·09	1·11	...	0·69
„ 21	0·89	1·000	0·96	0·98		1·09		1·07	...	1·07	1·25	...
„ 27	0·92	1·000	0·97	0·97		1·01		1·02	1·01	1·02	...	0·525
Means:	0·840	1·000	0·970	0·961		1·110		1·130	1·091	1·124	1·555	0·655

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Eclipses of Jupiter's Satellites in 1875, *at Willet's Point, N.Y.H., U.S.A.* By W. H. Bixby, Lieut. U.S. Engineers.

(*Communicated by E. B. Knobel.*)

Sat.	Phase.	Date and time of Observation. Month. d h m s	Assumed Chron. Correction. h m s	Corrected Willet's Point time of Observation. Sidereal. h m s	Mean Solar. h m s	Corresponding Washington M.S. Time. h m s	Correction to Am. Ephemeris. m s	Remarks.
		Mean Solar Chron. 596.						
I	Reap.	Apr. 20 08 29 54·20	02 24·37−	...	08 27 29·83	08 14 23·8	+01 41·8	*See* Note A.; Chron. Corr. obtained from 2 sets of 7 observations, each on ⊙.
II	Reap.	May 2 14 13 48·00	00 36·20−	...	14 13 11·8	14 00 05·8	−01 25·4	*See* Note B.; ditto ditto.
		Sidereal Chron. 217.						
I	Reap.	May 4 11 31 32·66	3 32 58·15+	15 04 30·81	12 13 50·4	12 00 44·0	+ 0 13·2	*See* Note C.; ditto ditto.
I	Reap.	May 13 08 28 59·00	3 33 32·07+	12 02 31·07	8 36 57·35	08 23 51·0	+ 0 46·0	Windy; instrument vibrating at observation; time obtained from 2 sets of 7 observations, each on ⊙.
		Sidereal Chron. 141.						
I	Reap.	May 27 16 20 22·00	25 44·7 +	16 46 06·7	12 24 43·8	12 11 37·4	+ 0 04·2	Time obtained from transit observations.

Note A.—Satellite I. is, at best, rather faint with this Zenith Telescope; in this particular observation the satellite first appeared as a bright protuberance on the edge of *Jupiter's* limb, and could not have been seen until it had reached its maximum brightness. (*See* Note C.)

Note B.—At time of observation I was very confident that the time of reappearance was noted and recorded correctly; it would have been difficult to have made a mistake of nearly a minute and a half in recording the time. I tried to get other observations on this satellite, but cloudy weather prevented.

Note C.—After recording the time of first appearance, note was made of the time which elapsed before the satellite appeared to be of maximum brightness; this interval, from first appearance, was about ($01^m\ 30^s$) one minute and thirty seconds.

Location of Willet's Point.—Longitude, $4^h\ 55^m\ 06^s{\cdot}04$ W. of Greenwich; Latitude, $40°\ 47'\ 20''{\cdot}52$ N.

Instrument.—Zenith Telescope: Aperture = 2·7 inches; focal length = 45·0 inches.

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Phenomena of Jupiter's Satellites, 1875–77.

By the Rev. S. J. Perry.

Owing to the loss of two assistants, one of whom was carried off by a rapid decline, it has been necessary during the last two years to confine our attention at this Observatory almost exclusively to the routine work which would suffer most by interruption. This, along with the exceptionally bad skies of the two last summers, will account for the fewness of the observations I am at present able to forward, but I send them in hopes that they may help to fill up some lacunæ in more ample records:—

Date.	Satellite.	Phenomenon.	G.M.T. h m s	Obs.	Remarks.
1875. Feb. 4	II	Tr. I. first contact	17 52 1·8	W.C.	Very unsteady, thin clouds.
		„ bisection	17 55 7·7	„	„ „
		„ last contact	17 58 5·9	„	„ „
Apr. 12	I	Tr. I. first contact	11 57 30·2	„	
		„ bisection	12 0 51·2	„	
		„ last contact	12 3 24·7	„	
„ 13	I	Ec. D.	9 18 32·7	„	Very close to disk; good.
	I	Occ. R. bisection	11 31 1·0	„	
		„ last contact	11 34 34·5	„	
1876. Mar. 16	II	Occ. R. bisection	17 46 1·7	„	Poor, clouds passing.
		„ last contact	17 50 46·9	„	
Apr. 7	I	Sh. I. bisection	13 2 56·4	„	Very faint.
	I	Tr. I. first contact	13 42 7·4	„	
		„ bisection	13 45 23·3	„	
		„ last contact	13 48 0·4	„	
	I	Sh. E. bisection	15 6 22·9	„	
„ 26	III	Ec. D.	11 46 21·8	„	Light fading 10m earlier; good.
	III	Ec. R.	13 21 14·4	„	Cloudy, fair.
May 3	III	Ec. D.	15 38 2·8	„	
June 1	I	Sh. I. first contact	9 45 19·4	S.P.	
	I	Tr. E. bisection	11 26 37·7	W.C.	
		„ last contact	11 30 36·4	„	
„ 25	I	Ec. R. first seen	9 20 56·3	S.P.	
		„ full light	9 22 2·0	„	
July 2	I	Ec. R.	11 15 34·5	W.C.	
„ 17	I	Tr. I. first contact	8 53 59·8	S.P.	Strong daylight.
		„ bisection	8 55 56·5	„	Definition good.
		„ last contact	8 58 50·0	„	

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Date. 1877.	Satellite.	Phenomenon.	G.M.T. h m s	Obs.	Remarks.
June 15	II	Ec. D.	10 32 3·6	W.C.	
	II	Occ. R. first contact	1 29 10·2	„	
		„ bisection	1 30 58·7	„	
		„ last contact	1 33 57·0	„	

Stonyhurst Observatory.

Observation of a Meteor. By J. L. McCance, Esq.

(*Communicated by Mr. Ranyard.*)

In the February number of the *Monthly Notices*, there is, under the heading "Progress of Meteoric Astronomy during 1876," a list of remarkable meteors observed during that year. Seeing that such a record is kept, I suppose that a short account of a fine meteor seen here lately may possibly have some interest. It appeared on Friday the 17th August, about 8.13 P.M., and its apparent path seems to have been one of about 15 degrees in length, beginning near β *Piscium*, at about R.A. $22^h\ 55^m$; Decl. 2° N., and disappearing at about R.A. $23^h\ 50^m$; Decl. 6° N., near ω in that constellation. It was a large round ball of bluish-green colour, and was as bright as the moon at First Quarter. The duration of its visibility was 2 to 3 seconds. In the *Standard* of the next Monday (the 20th) the (I presume) same meteor is mentioned as seen in the neighbourhood of Mornington Crescent, N.W.

Rathfern, Rayner's Road, Putney Hill, S.W.
1877, *August* 27.

On Jupiter's Third Satellite. By J. Tebbutt, Esq.

As my Notes on the physical peculiarities of *Jupiter's* third satellite, published in the *Notices* for December 1873, called forth interesting papers in the *Astronomische Nachrichten* from Professor Alexander and Dr. Klein, it may perhaps be well to record the following subsequent observations made by me of the same satellite.

The satellite was watched on March 4, 1874, but the beginning of the dark phase could not be determined, as the transit began before the planet rose above the horizon. The satellite reached its nearest approximation to the centre of the disk of its primary about $9^h\ 3^m$, being then only a faint dusky spot and not near so conspicuous as in the transits of April and May of the previous year. It was still extremely faint and near the edge of the disk at $9^h\ 55^m$. The Moon was only 2° or 3° from the planet. It must be observed that the chord traversed by the satellite was much shorter than in 1873.

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The satellite was again observed as a dusky spot on April 23, 1874. The last contact at ingress occurred about $6^h\ 55^m$, the definition being very bad. Six minutes and a half afterwards the satellite was still visible as a very faint luminous spot, but altogether disappeared by $7^h\ 9\frac{1}{2}^m$. At $7^h\ 37^m\ 44^s$, the definition having improved, it became visible as a faint dusky spot, and was soon very conspicuous. It was, however, not near so dark as in April and May 1873. It was seen with difficulty at $9^h\ 29\frac{1}{2}^m$ and became undistinguishable from the disk four minutes afterwards. The last contact at egress occurred about $9^h\ 53\frac{3}{4}^m$.

A transit of this satellite was again observed on July 18, 1874. The last contact at ingress occurred at $5^h\ 25\frac{1}{4}^m$. Occasionally between $5^h\ 56\frac{1}{4}^m$ and $6^h\ 4\frac{1}{2}^m$ I glimpsed a dusky spot near the estimated position of the satellite, but there was no doubt about its visibility at the latter time. The definition was excellent and the satellite soon became conspicuous, although it was projected on one of the faint belts of the planet. It was nearly as dark as the shadows in transit. At $8^h\ 10^m$ it was distinguished with difficulty and was invisible at $8^h\ 18\frac{1}{4}^m$. The bisection at egress took place about $8^h\ 28\frac{3}{4}^m$.

The transit of May 22, 1875, was watched without any trace of the dark phase being seen; indeed, from subsequent observations of transits when the apparent chord of the satellite's path was very short, there appears to be an absence of this phenomenon. This circumstance appears to favour the explanations of Dr. Klein in No. 2014 of the *Astronomische Nachrichten*.

I may mention that the rosy colour of the equatoreal belts of the planet was beautifully seen on May 22, 1875, and May 5, 1877.

Windsor, N.S. Wales,
October 17, 1877.

A New Astronomical Clock.

By C. V. Boys, Esq., Associate of the Royal School of Mines.

In bringing before your notice this new form of Astronomical Clock it will be well first of all to describe the main principles on which it is founded, and then, by describing the details, to show how those principles are carried out. Of course on the pendulum everything depends. The drawing shows a mercury compensation pendulum supported by a spring, but whatever form is thought best might be used. As perfect compensation for atmospheric changes, though now practically almost perfect, can theoretically never be attained, it is an advantage to have the pendulum in a vacuum. This is good for another reason: it requires much less driving power to keep it swinging, and to changes in this power many of the irregularities of clocks are due. Mr. Carrington had a clock in a vacuum which was

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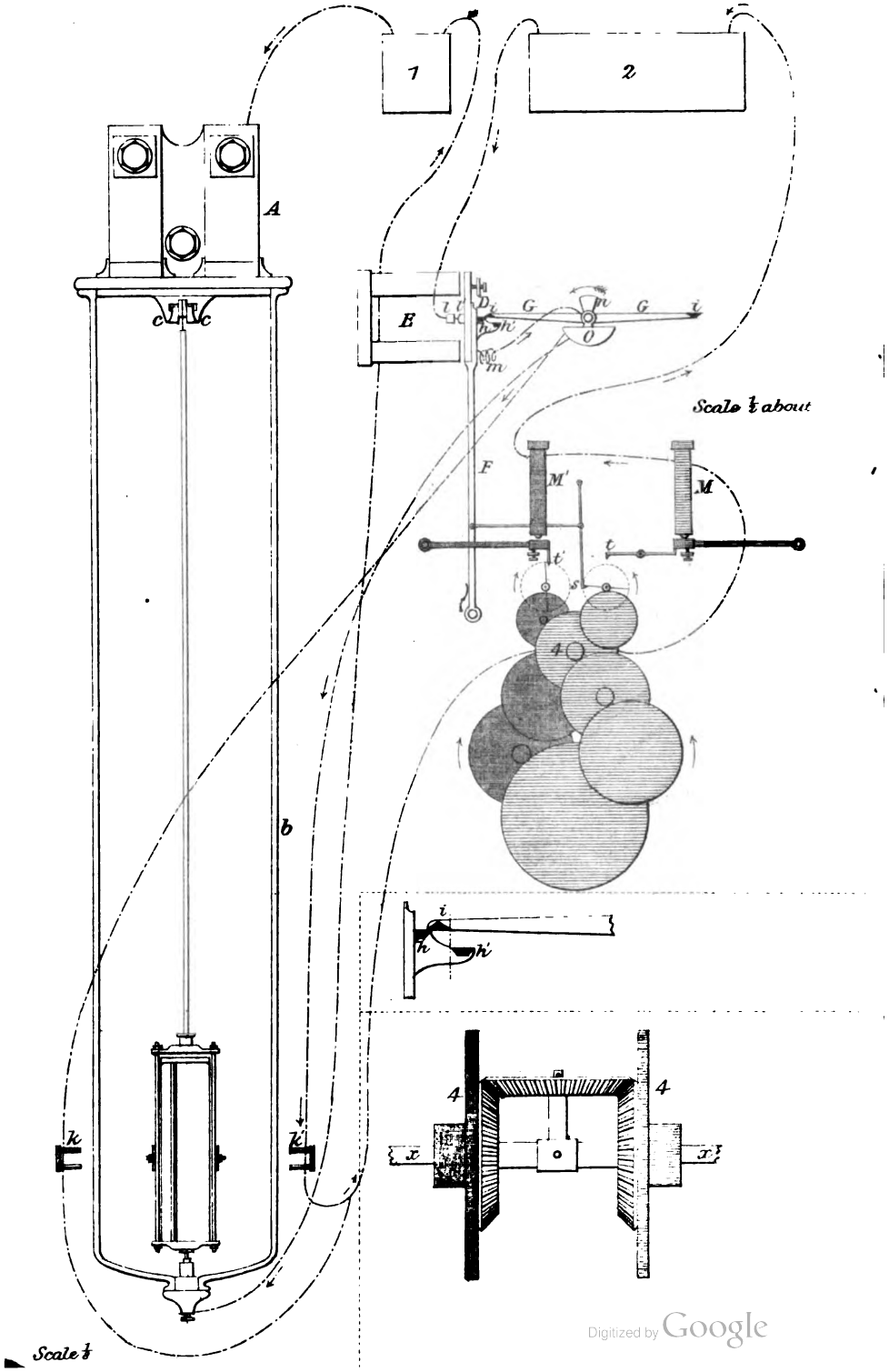

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wound up through a stuffing box; but this introduces a new error: the leakage at the stuffing box causes a slow increase of pressure inside the case, and this affects the uncorrected pendulum more than barometric changes in the air do a corrected one. Therefore, if the pendulum is in a vacuum, there must be no moving parts through the vacuum case. The impulse, which must, of course, be invariable, should be given to the pendulum while it is passing the middle of the swing; at all other times, especially at the ends of the oscillations, the pendulum should be absolutely free; also any work done by the pendulum, as unlocking pallets etc., should be at the middle of the swing. The impulse is usually given to the pendulum at a point about a quarter of the way from the top; but this is not the best place. An impulse, even if very powerful, given at the centre of oscillation would tend to turn the pendulum as a whole round its support; but if given anywhere else it would tend to bend the pendulum rod and to strain the supporting spring, and so would not act in the most perfect way. Lastly, a mechanical impulse cannot act so steadily as one due to an attractive force. In all these particulars the clock I am about to describe is as nearly as possible perfect; and, if what I have said is true, it should keep very accurate time.

I now come to the description of the clock itself. A is a powerful bracket secured to the wall; on the lower surface of this a ring is turned, to which the top of the glass case *bb* is secured. This case is afterwards to be exhausted. From the central projecting hooks *cc* hangs the pendulum, carrying at its lower end a small platinum disk, which, at the centre of each oscillation, touches a globule of mercury. 1 is a battery of low power, from which the current proceeds, first, to the supporting bracket, then down the pendulum rod to the mercury globule, round the electro-magnet E, and so back to the battery. Now it is evident that at the centre of each oscillation the electro-magnet E will attract its armature D. D is carried by an arm F, at the top of which is an escapement which the drawing sufficiently explains. The arm GG is driven by clockwork (not shown in the diagram) so that one of the triangular pins *i*, *i'* rests on one of the pallets *h*, *h'*. From what has been said it will be seen that at every swing of the pendulum the arm G will turn through 180°, that is, will turn 30 times in a minute; thus hands moved by the wheelwork driving the arm may be made to show hours, minutes, and seconds.

The impulse is given to the pendulum from outside the vacuum case in this way:—On each side of the pendulum is fixed a small piece of soft iron by a bolt in a line with the centre of oscillation of the pendulum. Outside the glass case are placed two small electro-magnets *k*, *k'*; if these can be alternately made magnetic the pendulum will be kept swinging. 2 is a battery as constant as possible, from which the current passes, first to the platinum block *l*, then to the platinum hammer *l'*, down the arm F, through the spring *m*, and on to the axis of the arm G. On this are

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fixed two platinum sectors n, n', side by side but in opposite directions. Below there are a pair of mercury troughs o. When the arm G is horizontal one of the platinum sectors is in its trough while the other is out, so, as the arm G turns round, each trough is alternately put in connection with battery 2. Each trough is connected by a wire with one of the electro-magnets k,k'; after passing them the two wires join, pass through the regulating mechanism, and back to the battery. Owing to the platinum pieces l, l' being in contact only when the armature D is attracted, that is, when the pendulum is passing its vertical position, it follows that the pendulum will receive its impulse only at this time; also the impulse will act in a line passing through the centre of oscillation of the pendulum. So far therefore the pendulum is under the conditions stated at first, namely, it swings in a vacuum and so is not affected by atmospheric changes, and it requires a smaller impulse to keep it swinging; there is no moving part through the vacuum case, which can therefore be made perfectly airtight. The impulse is given at the middle of each swing of the pendulum. The impulse is in a line with the centre of oscillation. The pendulum does work, in this case touches the mercury, only in the middle of the swing. The impulse is not given mechanically, but by the uniform action of an electro-magnet. There is one point, however, of the greatest importance not mentioned yet; it is how to keep the impulse, and therefore the current from the battery 2, constant. If at one time the impulse is greater than at another, the pendulum will swing through a larger arc and will therefore go more slowly; but if by any means the impulse can be kept constant, the pendulum will likewise swing through a constant arc. To keep a continuous current constant is an unsolved problem; but the current we have to deal with is intermittent, and though it cannot be kept absolutely constant, yet its practical equivalent can be attained, namely, that the current shall never vary beyond two limits, which limits can be brought as near together as we please. The method of electro-magnet and armature fails to keep a continuous current constant for this reason: the armature, in order to work the regulating machinery, must have a certain amount of play between its stops, and therefore, when at its nearest a decidedly weaker current will be sufficient to keep it attached than would be necessary to attract it when at its greatest distance; therefore fluctuations of a similar amount will take place in the strength of the current. But in an intermittent current the case is quite different: every time the current starts the armature is at the same distance from the electro-magnet, and the same strength of current therefore will be sufficient to attract it every time, but if the current is ever so little weaker the armature will not be attracted. Now, suppose there are two electro-magnets in the same circuit, one of which will for a given strength of current just attract its armature, while the other will just not; then, if the current is of the right strength, one armature only will be attracted, if too weak neither,

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if too strong both. These movements of the two armatures may be employed to turn a rheostat, through which the current passes, and so increase or diminish the total resistance according as the current is too strong or too weak. In the drawing, part of the regulating mechanism is coloured blue and part pink* ; the blue part is to regulate the current when too weak, and the pink part when too strong. M, M′ are the two electro-magnets on which the regulating action depends, so arranged that, when the current is right, the blue one only attracts its armature, when too weak neither, when too strong both. The effect of either electro-magnet on its armature depends on the dead weight of the latter, which is invariable, and on the distance, which can be regulated by a screw stop. This is better than using a spring, the strength of which changes with the temperature. This was suggested by my friend Mr. Liveing. Below these electro-magnets are two similar trains of wheels terminating in flies, and as they are to go in opposite directions, the same spring will drive them both.

Wheels 4 of each train run loose on the same axis x, but each carries a bevel wheel gearing with a third, as shown in the lower diagram, so either train is free to run while the other is still, but in so doing it carries the axis x round with half the velocity of its fourth wheel, and in the same direction, according to the formula $e = \frac{n-a}{m-a}$. The axis x turns the rheostat. The parts undescribed will be easily understood from the drawing. Suppose the current of the right strength: then, the moment the pendulum touches the mercury globule, D is attracted and the detent s is drawn back, but at the same time the electro-magnet M attracts its armature and the detent t prevents the blue train from running. The other electro-magnet only attracts its armature when the current is too strong, therefore the detent t' prevents the pink train from running, so both trains of wheels, and therefore the rheostat, keep still. Next suppose the current too weak. As before, the detent t' will be down; but the electro-magnet M, being unable to attract its armature, will leave the blue train free to revolve, so the rheostat will turn a little and diminish the resistance; but if the current is too strong both magnets will attract their armatures, the blue train will be stopped, and the pink one will be free; but it turns in the opposite direction to the blue train, therefore the rheostat will turn a little the other way and increase the resistance. If by any accident m' only were to act, both trains would be set running with an equal velocity in opposite directions and the rheostat would stand still. A single electro-magnet might be used instead of two, arranged to stop the blue train when attracted and the pink one when not attracted. This would keep the strength of the current oscillating on either side of the proper strength. There is no objection to the mercury arrangement $n\,o$, as when the circuit is made and broken the plate is fully

* The blue part is shown by faint shading, the pink part by dark shading.—Ed.

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immersed, so there will be no sparks and the resistance will be constant. It is important that the two wires from the mercury troughs *o*, to the electro-magnets *k*, *k'* and on to the junction should have the same resistance, for if not it would be impossible to get a uniform current from the battery 2. Nearly if not quite all the objections to the mercury contact at the lower end of the pendulum are here done away with. The battery 1 may be of very low electro-motive force; the circuit is short, so that there will be hardly any self-induction to produce sparks. The mercury globule being in a vacuum, and if thought well in a hydrogen vacuum, its surface cannot get oxidised. As to the shape of the globule, a circular one, that is, one with a circular horizontal section, would not be good, as, if by chance the pendulum should swing not always in the same plane, but sometimes nearer the front and sometimes nearer the back, the duration of contact would not be uniform, therefore the duration of impulse would vary, which would be fatal. A long globule, then, with parallel sides would be free from this objection.

There is one more point of importance, which is the regulation of the clock. The pendulum would be made as nearly right as possible by the ordinary methods, after which the final regulation could be obtained with the greatest nicety without touching or interfering with the pendulum in any way. As a pendulum swings more slowly in a large arc than in a small, it follows that, by putting the electro-magnets *k*, *k'* nearer to the pendulum, and so increasing their action on it, it would swing through a larger arc and therefore more slowly; and in the same way, by putting them further off, the pendulum would go faster. The two magnets *k*, *k'* would be capable of being moved towards or from the pendulum according as the clock gained or lost. Lastly, the movement of the arm F could be made to send any number of currents every second without in any way interfering with the clock.

Ninth Catalogue of New Double Stars discovered with the 6-inch Refractor. By S. W. Burnham.

The following list comprises the new pairs found with the 6-inch Clark Refractor in the latter part of 1876 and the first part of 1877. During this period most of the time was devoted to the examination of known double stars, and but little attention given to the discovery of new objects. Many of these have been since observed by Baron Dembowski and Professor A. Hall, and their measures are given here.

Since July of the present year this series of double star observations has been continued with the 18·5-inch Clark Refractor of the Dearborn Observatory, and it is hoped that in some respects at least the results obtained have a proportionately increased value.

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No.	Designation.	R.A. 1880. h m s	Decl. 1880. ° ′	Pos. °	Dist. ″	Mags.
453	Arg. (56°) 338	1 37 8	+56 31	218·7	1·· ±	9·0... 9·5
454	O² Arg. 8280	8 10 12	−30 29	18·6	2· ±	8·0...10·0
455	L 18231	9 8 33	+ 4 43	65·2	1·94	9·5...10·5
456	L 22020	11 30 44	−11 41	68·2	0·65	9·0... 9·0
457	O¹ Arg. 11836	11 55 15	−20 51	84·3	0·89	8·0... 9·0
458	L 22677	11 58 8	−20 22	240· ±	20· ±	8·0...10·0
459	W¹ XII. 689	12 41 58	+ 4 7	285·2	3·61	8·5...11·0
460	W¹ XIII. 273	13 18 40	−15 0	215·2	2·07	8·0...10·5
461	W¹ XIII. 850	13 50 36	+ 3 34	220· ±	20· ±	7·5...11·0
462	Lam 1702	14 23 46	− 3 11	324·4	2·01	9·5... 9·8
463	...	18 10 46	−16 55	...	1·3 ±	9·0... 9·3
464	W¹ XVIII. 476	18 21 45	+ 6 29	111·3	1·20	8·5... 9·5
465	Arg. (56°) 2130	18 41 38	+56 45	293·2	3·06	9·0...11·0
466	W¹ XVIII. 1503	18 59 34	+10 39	167·7	1·64	9·0...10·0
467	O¹ Arg. 19936	19 39 24	−21 49	140· ±	1·8 ±	8·0...10·5
468	L 37571	19 39 58	+ 3 57	182·4	9·55	7·0...11·3
469	W² XIX. 1757	19 54 28	+24 24	175·4	14·42	8·3...10·6
470	O² Arg. 20079	20 3 41	+63 25	213·8	2·32	9·5...11·0
471	Arg. (61°) 2046	20 41 1	+62 0	125·9	1·46	10·0...10·0
472	Arg. (61°) 2078	20 57 12	+61 23	6·5	0·66	8·3... 8·5
473	...	21 1 26	−10 42	115·4	1·70	9·0...10·0
474	O² Arg. 23373	22 1 2	+60 25	360· ±	10· ±	8·5...11·8
475	L 43305	22 6 15	− 8 36	240· ±	1·5 ±	7·5...11·0
476	W² XXII. 180	22 8 41	+30 48	93·1	2·62	9·5...10·0
477	W² XXII. 225	22 10 28	+30 49	45·9	6·42	9·2...11·0
478	Lam. 4578 A and B	22 23 8	− 7 56	32·6	1·26	10·0...10·0
	A and C			239·0	28·45	10·0
479	Arg. (67°) 1444	22 26 28	+67 33	29·8	2·42	9·7...11·2
480	W¹ XXII. 716	22 35 18	+ 4 6	63·1	0·79	9·5...10·0
481	W¹ XXII. 1162	22 56 23	−11 53	53·0	1·31	9·0... 9·5
482	Arg. (62°) 2350 A and B	23 55 44	+62 39	360· ±	4· ±	8·5...10·0
	A and C			150· ±	10· ±	11·0

Notes.

453. A very difficult pair, the distance of which was at first estimated 1″·2. Baron Dembowski could not see it double, and upon a re-examination with the 6-in. on two or three good nights I was unable to see any certain trace of duplicity. Subsequently I tried it with the 18½-in. of the Dearborn Observatory, and obtained a fair measure of the position-angle. I found the distance much overrated, and the esti-

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mate given above is probably still too large. It was a difficult object even with the large aperture, and impossible to measure in distance on this occasion. When discovered the entry made no mention of its being a particularly difficult pair.

454. Position-angle measured with the 6-in.

455. Measures by Prof. A. Hall (*Astr. Nach.* 2147), with the 26-in. (1877·3), on two nights. In Lalande this star is 7½ magnitude. When found it appeared to be about 8·5 mag.

456. Excessively difficult; the most northerly of two small stars in the same field. Measures by Prof. A. Hall (*Astr. Nach.* 2147), with the 26-in. (1877·3), on two nights.

457. Measures by Prof. A. Hall (*Astr. Nach.* 2147), on two nights, with the 26-in.

458. Noted in observing the preceding pair.

459. Measures by Baron Dembowski (1877·5); two observations.

460. Measures by Baron Dembowski (1877·4); two observations.

461. Very minute distant companion.

462. A pretty but faint pair, directly south of Σ 1852 *rej.* Measured on two nights by Baron Dembowski (1877·5).

463. Not in any star catalogue I have. Place with micrometer from H 2824.

464. A third star, 11 mag., 90° : 25″. Measures by Baron Dembowski; two nights (1877·6). In a low-power field with OΣ 350.

465. Measured once by Baron Dembowski (1876·8).

466. Measured once by Baron Dembowski (1876·7).

468. Found in looking for OΣ 381. Observed three times by Baron Dembowski (1876·9).

469. A mean of three observations by Baron Dembowski (1877·0).

470. In the field with Σ 2642. Measures by Baron Dembowski; one night (1876·8).

471. A pretty little pair; found in looking for H 3000. In Argelander 9·4 magnitude. Measured once by Baron Dembowski (1876·8).

472. The wide pair in the field, south, is H 1607. Measures by Baron Dembowski; two nights (1877·2).

473. Not found in any star catalogue I have. A mean of two measures by Baron Dembowski (1876·7).

475. Very unequal and difficult pair.

476. This and the next in the same low-power field. Both measured by Baron Dembowski on two nights (1876·8).

478. Measured once with the 18½-in (1877·8). Very difficult with the 6-in. There is a fourth smaller star, about the same distance as C in the opposite quadrant.

479. This makes, with two 8–9 magnitude stars, all in the field, a small equilateral triangle. A difficult object. Measured once by Baron Dembowski (1876·8).

480. Very difficult. Measures by Baron Dembowski; one night (1876·9).

481. Very difficult pair in the field with Σ 2970. Measures, one night, with the 18½-in. (1877·8).

482. For the close pair Baron Dembowski, by two observations, gives 342°·7 : 4″·07 : 10·0...11·0 (1877·2). The principal star in Argelander is 8–9 magnitude.

Chicago, Nov. 5, 1877.

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Extract from a Letter from the Rev. F. Howlett, to W. T. Lynn, dated 1877, November 3.

By this post I forward you careful drawings of the fine solar spot (now on the disk) for October 31 and November 1, taken by projection, power 120, and screen 5 ft. 2 in. from eye-piece, when each half-inch subtends just 15″. The main spot must have been very near the solar equator, especially when we consider that the Sun's north pole is now within the margin of the disk.

The lesser group "followed" the principal one after an interval of about 4′ 30″.

I would add that a friend who was watching me as I took the drawings noticed, as well as did I, a very distinct *haze* of a kind of bistre-brown colour extending for some 10″ or 12″ upon the penumbra, beyond the margin of the umbra, on October 31, as well also as a considerable sized nucleus of about 12″ diameter within the umbra.

The definition on October 31 was very fine, and it was very good also on November 1, though I could barely distinguish any of the bistre-brown haze or the nucleus on the latter day.

The "bridge," extending more than half-way across the umbra, was not very luminous on October 31, and still less so on November 1, and on each day aforesaid terminated in very faint flocculent matter.

To-day (November 3), this flocculent matter is very much feebler still, and is all that remains of the "bridge."

Note on a Special Case of "the Most Probable Result" of a Number of Observations. By J. M. Wilson, Esq., Rugby.

It is beyond the reach of all but pretty good mathematicians to follow the reasoning about "probable errors" and "the most probable results" of a number of discrepant observations. But there is one law of weighting observations which lends itself so easily to calculation that perhaps some of the non-mathematical readers of the *Notices* may be interested in seeing it. The result is not new, though it was quite new to me when I first observed it, some six or seven years ago, and I do not remember to have seen it since, except in Mr. J. W. L. Glaisher's paper in the *Memoirs* for 1872.

A number of numerical readings are made of some magnitude that is to be measured; they differ from one another. What is the most probable result? The common method is to take their arithmetical mean; and if the readings are tolerably evenly distributed this will not give a bad result. But if one reading is widely different from the others, the doubt occurs whether it ought to be wholly rejected or have less weight assigned to it.

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On the principle of the arithmetical mean there is no choice; the observation must be accepted wholly or rejected wholly, unless of course arbitrary weights are assigned to the observations by an *ex post facto* judgment of them.

But suppose we agree that observations shall have less weight the remoter they are from the final result; in fact that the weight of an observation shall vary inversely as its error from the result; so that, if our final result is 27·4, an observation 27·5 shall have more weight than an observation 27·9 in the ratio of 5 to 1, and more than an observation 28·4 in the ratio of 10 to 1.

The most probable result of a number of readings will, according to this law of weighting observations, be obtained as follows:—

Let the observations be arranged in order of magnitude; *then, if the number of them is odd the middle observation must be taken; and, if the number is even, anywhere between the two middle observations must be taken, supposing all to be unequal.*

For the case is this. Let a rod without weight have a number of particles resting on it having weights each inversely proportional to their distances from the point of suspension. Find the point on which it will balance.

If the distance of any particle from the point of suspension is x and its weight $\frac{1}{x}$, then its moment is 1, and all that is necessary is that there shall be an equal number of particles on both sides of the fulcrum.

Hence if a number of readings of the distance, we will suppose of a double star, are taken as follows: 27·3, 29, 28·3, 26, 26·8, 27·1, 28, 28·5, 27·4, they must first be arranged in order of magnitude, 29, 28·5, 28·3, 28, 27·4, 27·3, 27·1, 26·8, 26; and, the number of them being odd, the middle one, 27·4, is the most probable result. The arithmetical mean is 27·6.

If the last observation were cancelled, the most probable result would be anywhere between 28 and 27·4; the arithmetical mean is 27·8.

There is one curious case in which this method gives apparently *no* most probable result. This case would have arisen if 27·3 is substituted for 27·4. The most probable result cannot be 27·3, for there then would be three readings on one side of it and four on the other; nor can it be more than 27·3, for then there would be five readings on one side and four on the other. Of course the solution of the paradox is to be found by supposing the two equal readings to differ by an infinitesimal amount, and the real result would therefore be 27·3.

Rugby, Nov. 1877.

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Note upon the Determination of the Longitudes of Kurrachee and Madras. By Major-Gen. Thomas Addison, C.B.

In 1874 arrangements were made for exchange of galvanic signals for difference of longitude between the German Observers of the Transit of *Venus* at Ispahan and my station at Kurrachee, and also between the latter and the Madras Observatory.

Between the two former stations 225 signals were exchanged on the 11th and 12th December, and on the 13th 200 between Madras and Kurrachee.

Professor Auwers has favoured me with the results of the first sets, and Mr. Pogson did the same in the case of those with him. They are

Difference of Longitude.

	h	m	s
Ispahan—Kurrachee	1	1	13·09
Kurrachee—Madras	0	53	6·22

Dr. Becker, of the Royal Observatory, Berlin, has further kindly communicated to me the difference of longitude, similarly obtained, between Berlin and Ispahan; and as that between Greenwich and Berlin is given in the *Nautical Almanac*, the whole chain is thus completed from Greenwich to Madras. It stands thus.

		h	m	s
Berlin, east of Greenwich	By *Nautical Almanac*	0	53	34·9
Ispahan, east of Berlin	By galvanic signals by Dr. Becker and Dr. Fritsch	2	33	5·44
Kurrachee, east of Ispahan	By galvanic signals between Drs. Becker and Fritsch and General Addison	1	1	13·09
Madras, east of Kurrachee	By galvanic signals between Mr. Pogson and General Addison	0	53	6·22
Madras, east of Greenwich		5	20	59·65

Both Professor Auwers and Mr. Pogson expressed themselves well satisfied regarding the reliableness of the signalling operations, and I feel every confidence in the final result as concerns Madras.

That for Kurrachee has an element of doubt in it, inasmuch as I neglected to obtain the pivot error of my Transit Instrument. But to whatever extent the pivot error affected the result as regards Kurrachee, the difference between Ispahan and Madras remains unaffected, the errors on the east and west sides of Kurrachee correcting each other.

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In the *Monthly Notices* of the Royal Astronomical Society for March 1872 are recorded the results obtained by Colonel Walker and Major St. John by telegraph between Teheran and Greenwich, from which was deduced the longitude of Teheran, viz.

$$51^\circ\ 24'\ 26''\quad \text{or}\quad 3^h\ 25^m\ 39^s{\cdot}73.$$

Major St. John had previously obtained the difference Teheran—Kurrachee. He does not give it, but he states that these two operations, combined with those of the great Trigonometrical Survey of India, bring out the longitude of Madras within half a minute of arc of that given in the *Nautical Almanac*.

The latter is $5^h\ 20^m\ 57^s{\cdot}3$; therefore he must have made the difference Teheran—Kurrachee $1^h\ 55^m\ 17^s{\cdot}57 \pm 2^s$.

In 1874 Dr. Becker determined the difference between Teheran and Ispahan, and this affords us the means of comparing the operations of 1872 and those of 1874.

According to Dr. Becker the difference Teheran—Ispahan is $58^s{\cdot}8$, and as Ispahan—Madras is $1^h\ 54^m\ 19^s{\cdot}3$, there results for Teheran—Kurrachee, by the operations of 1874, $1^h\ 55^m\ 18^s{\cdot}11$, from which Major St. John differs by $\cdot 54^s \pm 2^s$. The local time was obtained by Colonel Walker, by Sextant, that by Dr. Becker by Transit Instrument.

The longitude of Teheran obtained by Colonel Walker does not quite agree with that obtained by Dr. Becker. The latter was

	h	m	s
Berlin—Teheran	2	32	6·68
Long. Berlin by *Nautical Almanac*	0	53	34·9
Resulting longitude of Teheran	3	25	41·58,

from which Colonel Walker differs by $1^s{\cdot}85$.

At Kurrachee my Observatory was $0^s{\cdot}6$ east of that used in the great Trigonometrical Survey of India at that place. Major St. John's exact station is not given.

The Hill House, Melton, Suffolk,
December 4, 1877.

Extract from a Letter from R. J. Ellery, Esq., to the Astronomer Royal, dated Melbourne, 1877, October 3.

My Dear Sir George,—Your telegram of the 17th August relative to suspected satellites of *Mars* reached me on the 22nd, and we have taken every opportunity since of searching, but as yet without success. Unfortunately the great telescope has been *hors de combat*, part of the declination movement having broken. This is now nearly repaired, and in the meantime the

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watch has been kept with the 8″ refractor. Our scrutiny assumes the satellites (if they exist) to be very faint objects, and we are obliged to eclipse *Mars* itself to enable us to take our search close to his limb.

Extract from a Letter from R. J. Ellery, Esq., to the Astronomer Royal, dated Observatory, 1877, October 31.

My Dear Sir George,—We continued our search for *Mars'* satellites up to the 27th instant, but, with the exception of one occasion, upon which *we believe* we saw one of them, our search has been fruitless. The occasion referred to was on the night of the 16th October, when *Mars* occulted a star of about the 13th magnitude at $22^h\ 15^m$ (Sid. Time); after emergence a very faint point (20th magnitude estimated) was seen half a diameter from *Mars* s.p.; this was watched for nearly an hour, when its position indicated a motion with *Mars*, but before any verifying measures could be made the sky became cloudy, and no other signs of satellites have been observed since. Our first search with the great telescope was on the 26th September, and it is very probable our want of success is due to our lateness in the field.

We have obtained a fine series of observations for the parallax of *Mars* with our Transit Circle.

The Opposition of Mars, 1877. By Maxwell Hall, Esq.

(*Abstract.*)

Observations of *Mars* were made in Jamaica during the last opposition of the planet, in order to determine the Solar Parallax by means of the displacement of the planet in Right Ascension, when far east and far west of the meridian as seen at a single place.

The small Observatory is situate about $5\frac{3}{4}$ miles south-east of Montego Bay, on a range of hills 1800 feet above the sea-level. The buildings consist of a square Transit-room and a circular tower for the Equatoreal. The piers of the instruments are founded on solid rock, and the whole of the mason-work is compact and substantial.

The instruments were made by Messrs. Cooke & Sons, of York, and the mounting of the Equatoreal is very firm and massive. The aperture of the object-glass is only 4 inches, but the definition is very good. The transit eye-piece has five wires about 2′ 45″ apart, and a power of 120 was employed. The Transit Instrument has an aperture of 3 inches. The clock is provided with a mercurial compensating pendulum and the dead-beat escapement; it was an inexpensive instrument (cost-

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ing only £35), being in fact chiefly intended for adjusting the Equatoreal. The daily change of rate was generally small, $0^{s}\cdot5$ or thereabouts, but there was great irregularity of rate: for instance, an interval of 120 seconds between two stars is generally measured by $119^{sec}\cdot9$ and $120^{sec}\cdot1$ alternately; but often there is far greater irregularity, and differences of $0^{sec}\cdot2$, $0^{sec}\cdot3$, and $0^{sec}\cdot4$ occur between consecutive measures of such an interval.

The observations were commenced on the 4th of August, and were continued, with but few interruptions, up to September 17, when the weather broke up. The observed stars are numbered according to the list given in the *Monthly Notices*, November 1875, the only additional star employed being one of the $7\frac{1}{2}$ mag., referred to as *c* (see *Nature*, December 14, 1876). The work is divided into four sections: the first relating to the adjustment of the Equatoreal; the second to the reduction of the observations; the third containing the observations themselves; and the last the determination of the Solar Parallax in accordance with the previous sections.

By applying the weights as originally determined, the value obtained for the Solar Parallax was $8''\cdot80$. But, dividing the observations into two classes—the first class containing those which were made with two comparison stars, and the second class those made with only one comparison star—it was found that the mean value of $(\text{error} \times \text{weight})^2$ was about five times as great for the second class as for the first. Consequently the weight due to the quality of the first method is to the weight due to the quality of the second method as 5 : 1 nearly; but this result depending very much on the observations made on one night, it was thought advisable to reduce the ratio, and the ratio adopted was 4 : 1; and multiplying accordingly the former weights of the first class by 4, the resulting Solar Parallax was

$$8''\cdot789,$$

with a probable error of $0''\cdot060$.

Dun Echt Observatory Publications, Vol. II. Mauritius Expedition, 1874. *Division I. Determination of the Solar Parallax by observations of the Minor Planet Juno* (4) *at Opposition, together with a description of the Heliometer used in the observations.* Pp. i. to xiv. and 1 to 212.

It is proposed to give in another volume full reports of the chronometric and telegraphic determinations of the longitude of Mauritius, together with the observations for latitude and time which were made during Lord Lindsay's stay in the island; and

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also to defer to a fourth volume the details of the observations of the Transit of *Venus* proper, comprising those made with the heliometer and the 6-inch Equatoreal and the accounts of the photographic arrangements and discussion of the negative obtained. The present volume relates to the determination of the Solar Parallax by the method described in the paper by Lord Lindsay and Mr. Gill (*Monthly Notices*, vol. xxxiv., p. 279). The method in its simplest form consists in measuring the position-angle and the distance of a minor planet relative to a star in the evening, and similarly in the morning. The difference of results, corrected for the motion of the planet, gives the data for computing the horizontal parallax. The total parallactic displacement is very considerably less than in the case of an opposition of *Mars* or a transit of *Venus*, but the authors believed that the great accuracy attainable in the heliometric measurement of the distance of two minute points of light would more than compensate for that defect.

They think that these observations have been justified by the result; and, as the best method for a rigid determination of the Solar Parallax is one of the most important as well as the most generally interesting questions presented to the astronomer, they thought it best to submit the present investigation as first fruits of the expedition.

It is distinctly to be understood that *the result arrived at is not presented as an example of the accuracy of which the method is capable, but is to be regarded merely as a first imperfect experiment, made under somewhat unfavourable and trying circumstances, from which conclusions may be drawn as to the capabilities of the method for the future.*

It was intended that the observations of *Juno* should commence on October 10, and the necessary stars of comparison were selected, and the Right Ascension and Declination of the middle point between star and planet, as well as the position-angle and distance of the star of comparison from the planet, were computed and published (*Monthly Notices*, vol. xxxiv., pp. 296–300) for each evening and morning between that date and November 30.

Unfortunately the yacht "Venus" with the instruments did not reach Port Louis till November 2, and it was November 5 before the heliometer could be unpacked at Belmont. On the 7th the instrument was completely erected and adjusted by the Sun and stars in daylight, and the adjustments, though tested frequently afterwards, were not again touched until the instrument was dismounted. Owing to cloudy weather, the first observations were made only on the night of the 10th, but under circumstances so unfavourable as to be nearly worthless. The first series of value was made on November 12, and observations were secured altogether on twelve evenings and eleven mornings between that date and November 30. Many of these were by no means so complete as desirable, as appearing from the recorded observations themselves and the notes attached to them. *Juno*

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being past opposition (opposition was on November 5) the object was to secure measures as soon as possible after sunset.

In correcting the equations for errors in the tables of *Juno* it was found that there was a discordance between the Washington and Cambridge (U.S.) observations of the planet on the one side and the Greenwich observations on the other; and it was considered best to reduce the observations on both systems. This was accordingly done, and in the final equations between Δa, $\Delta\delta$, z, and $\Delta\pi$, the values of the constant term are given according to these two systems respectively. The assumed value of the Solar Parallax was $8''{\cdot}85$, and applying hereto the values of $\Delta\pi$ coming out from the equations, the resulting Mean Solar Parallax comes out—

1st System.	2nd System.
$8''{\cdot}77 \pm 0''{\cdot}041$.	$8''{\cdot}76 \pm 0''{\cdot}042$.

It is stated that the values of these probable errors depend upon too few observations to be trustworthy, and that the authors do not themselves attach high importance to the deduced value of the parallax. But that to which they do attach high importance is the very remarkable accuracy of the method as shown from the residuals when the value in question is substituted in the equations. From the short table given it appears that the resulting probable error for a complete observation is on the first system $\pm 0''{\cdot}073$, and on the second system $\pm 0''{\cdot}082$, confirming beyond doubt the conclusion arrived at in the paper in the *Monthly Notices*, that it is possible, by means of the heliometer, to determine the place of a minor planet relative to two stars (or, if within a distance of $1000''$, to one star) with a probable error of less than one-tenth of a second of arc.

Accounts have been received of the Lunar Eclipse of the 23rd of August 1877, as observed at Stonyhurst by the Rev. S. J. Perry, at Brighton by Mr. H. Pratt, at St. Leonards-on-Sea by Mr. J. E. H. Peyton, and at Morges, Switzerland, by Mr. V. Fasel.

ERRATA.

p. 40, line 1, for "Wills" read "With."

Vol. xxxiii. p. 100, line 9 from bottom, for $46°\ 30'\ 0''$ read $46°\ 30'\ 36''$.
" " 8 " " $6°\ 31'\ 0''$ " $6°\ 29'\ 0''$.
" " " " " $0^h\ 26^m\ 2^s$ " $0^h\ 25^m\ 56^s$.

Vol. xxxiv. p. 425, line 9 from bottom, for $46°\ 30'\ 0''$ read $46°\ 30'\ 36''$.
" " 8 " " $6°\ 31'\ 0''$ " $6°\ 29'\ 0''$.
" " " " " $0^h\ 26^m\ 2^s$ " $0^h\ 25^m\ 56^s$.
fast of Greenwich.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. *January* 11, 1878. No. 3.

WILLIAM HUGGINS, ESQ., F.R.S., President, in the Chair.

The Rev. William H. Allen, 3 Kelly Street, Kentish Town Road, N.W.;
T. H. E. C. Espin, Esq., Wallasey Rectory, Birkenhead;
The Rev. David Fleming, Coxhoe Vicarage, Co. Durham;
F. R. Janisch, Esq., C.M.G., Governor of St. Helena;
Lieut.-Col. H. S. Knight, Army and Navy Club, S.W.;
Albert Marth, Esq., 66 Lambeth Road, S.W.;
George Francis Riddiford, Esq., Barnwood Lodge, near Gloucester;
The Rev. Philip R. Sleeman, Clifton, Bristol; and
The Rev. G. E. Watts, Kensworth Vicarage, Dunstable;

were balloted for, and duly elected Fellows of the Society.

Mr. D. Gill's Expedition to Ascension. Report, December 3, 1877.

Since my last report the weather has been much more unfavourable than any I have yet experienced here.

With an unceasing watch up to December 2, only three evening sets and one morning set of observations of *Melpomene* could be secured, and all were more or less imperfect.

The first half of opposition thus lost was, both as to distance of planet from the Earth and choice of comparison stars, far better than the second half; the weather shows no sign of improvement, and even under favourable circumstances it would be impossible now to secure what I should consider a satisfactory series of observations from which to find corrections of the planet's tables, and to deduce a value of the parallax. I have therefore decided not to attempt further observations of *Melpomene*.

I propose to devote the remainder of my stay to latitude and longitude determinations, and such other work as I can

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accomplish before Christmas. After that I propose to dismantle the Observatory, pack the instruments, and return by the January mail. I forward along with this report a copy of observations in the triangulation of the *Mars* stars of comparison. I am happy to think that the main objects of the expedition are thus accomplished.

Mars Bay, Ascension Island,
December 3, 1877.

Note sur les mouvements réels des étoiles dans l'espace.

By Señor V. Ventosa.

(*Communicated by W. H. M. Christie.*)

L'un des problèmes les plus intéressants qu'ait suscités l'application du spectroscope à l'étude des corps célestes est, sans doute, la détermination des mouvements des étoiles dans la direction du rayon visuel, au moyen du déplacement observé des raies de leur spectre. Plusieurs astronomes ont énoncé l'idée d'une relation, susceptible d'être exprimée par l'analyse mathématique, et qui nécessairement doit exister entre le mouvement *radial*, ainsi obtenu, et le mouvement angulaire apparent d'une étoile, puisque les deux ne sont que les composants du mouvement relatif de celle-ci dans l'espace. La Société Royale Astronomique de Londres a donné son opinion autorisée sur le nouveau procédé, "dont les résultats positifs—dit-elle—lorsqu'ils seront combinés avec les mouvements propres ordinairement observés dans les étoiles, nous fourniront des renseignements sur la vraie constitution du ciel." On a même indiqué quelques applications des mouvements radials à divers problèmes, tels que la détermination de la parallaxe des étoiles doubles; mais encore je ne connais aucun travail où la relation mentionnée ait été envisagée d'une manière générale.

Un Mémoire qui paraîtra bientôt contient les premiers résultats des recherches que j'ai poursuivies dans cette voie depuis Avril 1873, autant que les circonstances me l'ont permis, et dont je me propose de faire ici un court résumé.

Dans le mouvement relatif d'une étoile, le seul que nous puissions connaître par l'observation directe, et en supposant d'ailleurs, comme d'habitude, l'observateur placé au centre du soleil, il faut naturellement considérer les mouvements absolus de ces deux astres, en grandeur et en direction, leur distance mutuelle, et les mouvements radial et angulaire observés de l'étoile, ainsi que son lieu apparent sur la sphère céleste. Quelques-unes de ces quantités devant être exprimées en unités linéaires, tandis que les autres le seront en mesure angulaire, j'ai tâché, pour plus de clarté, de distinguer les premières par les caractères de notre alphabet, et les dernières par des lettres

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grecques, en y conservant du reste, lorsqu'il a été possible, la notation établie et sanctionnée par d'éminents astronomes.

Voici, d'abord, comment j'obtiens les équations fondamentales. Le triangle sphérique formé par le pôle de l'équateur céleste, le point du ciel (apex) vers lequel se dirige notre soleil, et une étoile, fournit les expressions connues :

$$\left.\begin{aligned} \sin\psi \sin\chi &= \sin(\alpha-\alpha_o)\cos\delta_o, \\ \cos\psi \sin\chi &= \cos(\alpha-\alpha_o)\cos\delta_o \sin\delta - \sin\delta_o\cos\delta, \\ \cos\chi &= \cos(\alpha-\alpha_o)\cos\delta_o\cos\delta + \sin\delta_o\sin\delta, \end{aligned}\right\} \quad (A)$$

où α, δ, α_o, δ_o sont respectivement l'ascension droite et la déclinaison de l'étoile, et du sommet parallactique ; $180°-\psi$ l'angle à l'étoile, et χ la distance de celle-ci au sommet.

Le mouvement propre angulaire ρ, déterminé par les mouvements en ascension droite et en déclinaison, et projeté sur le cercle parallactique χ, donne aussi, comme l'on sait,

$$\left.\begin{aligned} \rho\sin\psi_1 &= \Delta\alpha\cos\delta, & \beta &= \rho\sin(\psi-\psi_1), \\ \rho\cos\psi_1 &= \Delta\delta, & \gamma &= \rho\cos(\psi-\psi_1), \end{aligned}\right\} \quad (B)$$

où γ est la projection du mouvement et β la composante perpendiculaire.

Considérons maintenant les relations dans l'espace. Soient deux positions successives du soleil et de l'étoile aux instants t et t', suffisamment rapprochés entre eux pour que l'on puisse, dans l'intervalle, prendre comme rectilignes les déplacements élémentaires des deux astres. Concevons en outre un système de coordonnées rectangulaires, dont, pour plus de simplicité, nous placerons l'origine en coïncidence avec la position initiale du Soleil, et ensuite nous en ferons passer l'un des axes par le deuxième lieu du même astre, et l'un des plans (le *plan parallactique*) par la première position de l'étoile. Ordinairement les quatre points considérés ne seront pas situés dans le même plan ; donc si on les projette sur les axes coordonnés, on trouvera facilement les équations suivantes :

$$\left.\begin{aligned} r\cos\chi + n\cos\zeta\cos(\chi+\theta) &= m + r'\cos\beta\cos(\chi+\gamma), \\ r\sin\chi + n\cos\zeta\sin(\chi+\theta) &= r'\cos\beta\sin(\chi+\gamma), \\ n\sin\zeta &= r'\sin\beta, \end{aligned}\right\} \quad (C)$$

en appelant m et n les éléments rectilignes des trajectoires du Soleil et de l'étoile ; r et r' les distances entre ces deux corps aux instants t et t' ; ζ l'angle que fait la direction de l'étoile dans l'espace avec sa projection sur le plan parallactique ; et θ l'angle formé dans ce plan par la même projection et le rayon visuel initial.

Ces formules sont rigoureuses, mais β et γ étant toujours deux angles très-petits, on peut en déduire d'autres suffisamment approchées et qui offriront plus d'utilité. Pour cela on y mettra $\sin\beta=\beta\sin 1''$, $\cos\beta=1$, $\sin\gamma=\gamma\sin 1''$, $\cos\gamma=1$; et après

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des transformations très-simples, si l'on y fait $r'-r=s$, $r'=r$ (dans les termes où r' multipliera soit γ, soit β), et, enfin, $r \sin 1'' = p$, il en résultera :

$$\left.\begin{aligned} n \cos\zeta \cos(\chi+\theta) &= -p\gamma \sin\chi + s\cos\chi + m, \\ n \cos\zeta \sin(\chi+\theta) &= p\gamma \cos\chi + s \sin\chi, \\ n \sin\zeta &= p\beta. \end{aligned}\right\} \qquad (D)$$

La quantité indiquée par s (initial du *spectrum*) n'est que le *mouvement radial* observé, et p, qu'on pourrait appeler *distance réduite*, me paraît une sorte de mesure très-convenable pour les grandeurs sidérales. Les équations (D) peuvent encore recevoir la forme plus simple que voici :

$$\left.\begin{aligned} n \cos\zeta \cos\theta &= s + m\cos\chi, \\ n \cos\zeta \sin\theta &= p\gamma - m \sin\chi, \\ n \sin\zeta &= p\beta. \end{aligned}\right\} \qquad (E)$$

Dans les deux systèmes (D) et (E) on doit exprimer m, n, p, s, en rayons moyens de l'orbite terrestre : β et γ, qui ne représentent plus que des nombres abstraits, doivent l'être en secondes d'arc. Nous avons pris l'*année* pour unité de temps, en sorte que si s est donné en milles anglaises par seconde, il faudra le multiplier par $\frac{t'-t}{a} = 0{\cdot}3431$, a étant la distance moyenne de la Terre au Soleil.

Lorsqu'il s'agit de comparer entre eux les mouvements de deux étoiles dans l'espace, il faut employer de nouvelles expressions. Le même triangle sphérique auparavant considéré donne aussi les équations suivantes :

$$\left.\begin{aligned} \sin\omega \sin\chi &= \sin(\alpha-\alpha_o)\cos\delta, \\ \cos\omega \sin\chi &= -\cos(\alpha-\alpha_o)\cos\delta\sin\delta_o + \sin\delta\cos\delta_o, \\ \cos\chi &= \cos(\alpha-\alpha_o)\cos\delta\cos\delta + \sin\delta\sin\delta_o, \end{aligned}\right\} \qquad (F)$$

où ω est l'angle au sommet parallactique. Menons par le centre du Soleil des parallèles aux directions *actuelles* des trajectoires des étoiles : chaque parallèle coupera la sphère céleste en un point, qui pourra être regardé comme une nouvelle étoile. En appliquant, donc, les mêmes formules (F) à ce point, si l'on y distingue avec un accent les quantités analogues, on trouvera :

$$\left.\begin{aligned} \sin(\alpha'-\alpha_o)\cos\delta' &= \sin\omega'\sin\chi', \\ \cos(\alpha'-\alpha_o)\cos\delta' &= -\cos\omega'\sin\chi'\sin\delta_o + \cos\chi'\cos\delta_o, \\ \sin\delta' &= \cos\omega'\sin\chi'\cos\delta_o + \cos\chi'\sin\delta_o. \end{aligned}\right\} \qquad (G)$$

D'après cela, ω' et χ' une fois connues, on obtiendrait les coordonnées α' et δ' de la trace de la trajectoire. Or, il n'est pas difficile de voir que si l'on projette cette trace sur la corde parallactique de l'étoile, on aura finalement :

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$$\left.\begin{aligned} \sin(\omega-\omega')\sin\chi' &= \sin\zeta, \\ \cos(\omega-\omega')\sin\chi' &= \cos\zeta\sin(\chi+\theta), \\ \cos\chi' &= \cos\zeta\cos(\chi+\theta). \end{aligned}\right\} \qquad (H)$$

Mon Mémoire contient quelques exemples numériques détaillés de l'emploi des formules précédentes. Je ne peux que mettre ici les résultats obtenus avec l'étoile α *Lyræ*. En prenant pour données les nombres :

$\alpha_0 = 259° 52'$	$\alpha = 277° 58'$	$\Delta\alpha = +0''.255$
$\delta_0 = +32° 29'$	$\delta = +38° 39'$	$\Delta\delta = +0''.28$
$m = 1{,}623$	$\pi = 0''.180$	$s = -36.6$ (milles)

il viendra successivement

$\psi_1 = 35° 29'$	$\rho = 0''.344$	$s = -12{,}560$	$\theta = 174° 24'$	$\alpha' = 93° 49$
$\psi = 72° 44'$	$\beta = +0''.208$	$p = 5{,}550$	$\zeta = +5° 58'$	$\delta' = -31° 12$
$\chi = 15° 56'$	$\gamma = +0''.274$	$n = 11{,}112$	$\chi' = 168° 5'$	
$\omega = 62° 9'$			$\omega' = 272° 25'$	

Il s'ensuit, qu'α *Lyræ* se mouvrait dans l'espace en direction presque opposée et avec une vitesse beaucoup plus grande que le Soleil. Ces conclusions supposent, toutefois, exactement connues toutes les données numériques du problème, qui en tel cas serait déterminé. Mais, en réalité, on ignore la parallaxe de la plupart des étoiles, et par conséquent, les valeurs de r et p ; et celles qu'on a trouvées par l'observation ne sont pas jusqu'à présent très-sûres. En outre, les distances relatives basées en diverses hypothèses sur la grandeur apparente des étoiles, ainsi que sur leurs mouvements propres angulaires, sont encore l'objet de continuels débats. D'ailleurs, si l'on peut admettre comme assez connue la direction du mouvement du Soleil, ce n'est pas de même de la vitesse linéaire m de ce mouvement, qui dépend des mêmes distances hypothétiques ; en sorte que dans nos formules il faudrait considérer m et p comme des inconnues, avec n, ζ et θ. Cependant, d'une discussion qu'il n'est pas possible de reproduire ici, il résulte que m, n et p devant être des quantités essentiellement positives et leurs valeurs numériques comprises entre des limites convenables, ces conditions, et d'autres provenant de la structure même des équations fondamentales, permettront, dans la plupart des cas, de déterminer approximativement la région de l'espace vers laquelle une étoile se meut, au moyen de ses mouvements propres observés, radial et angulaire.

Un chapitre du Mémoire est consacré à la question importante du mouvement du Soleil. En effet, tous les procédés employés jusqu'à ce jour pour cette recherche découlent de quelques-unes des formules précédentes, et celui habilement développé par MM. Airy et Dunkin peut être déduit d'une manière simple des deux dernières équations (E), en substituant χ, β et γ dans (A) et (B). La première équation (E) me paraît très-convenable

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pour la solution du même problème, puisqu'elle ne contient que le mouvement radial et ne dépend pas des distances des étoiles. Après des substitutions faciles elle prend la forme :

$$s' = s + x_0 \cos\alpha \cos\delta + y_0 \sin\alpha \cos\delta + z_0 \sin\delta, \qquad (I)$$

où

$$x_0 = m \cos\alpha_0 \cos\delta_0, \quad y_0 = m \sin\alpha_0 \cos\delta_0, \quad z_0 = m \sin\delta_0, \text{ et } s' = n \cos\zeta \cos\theta.$$

s' n'est que la partie du mouvement radial due au mouvement réel de l'étoile, ou, ce qui revient au même, la projection de ce mouvement sur le rayon visuel.

En supposant, comme à l'ordinaire, que les mouvements particuliers des étoiles ont lieu indifféremment dans tous les sens possibles, je considère s' comme une erreur accidentelle qui doit disparaître des équations normales. Ensuite j'examine l'effet des erreurs constantes dans la détermination de α_0, δ_0 et m, et je fais séparément l'application du procédé aux séries numériques des mouvements radials trouvés à Greenwich et par Mr. Huggins. Les résultats ainsi obtenus sont, à mon avis, remarquables, malgré le petit nombre des étoiles employées, leur inégale répartition sur la sphère céleste, et l'existence possible d'erreurs systématiques dans les observations. Je ne crois pas, pourtant, que les anciennes méthodes doivent être abandonnées ; la comparaison de leurs résultats avec ceux que donne la méthode que j'ose proposer pouvant, peut-être, conduire à nous donner quelques renseignements sur le mouvement d'ensemble du système sidéral.

Observatoire de Madrid,
le 20 Décembre 1877.

On the Distribution of the Fixed Stars in Space.

By Sidney Waters, Esq.

In Mr. Proctor's Chart of all the stars in Argelander's maps, we have an accurate representation of the distribution of the stars of the northern heavens down to the ninth magnitude ; but we have no illustration of a similar character for the southern heavens. There does not indeed as yet exist sufficient data for the construction of a chart to correspond to Mr. Proctor's ; but I thought that it might be worth while, in the meantime, to utilise as far as possible the results of Sir John Herschel's star gauges given in his Cape of Good Hope observations.

I have therefore, in the chart now laid before the Society, represented on an equal surface projection these results in as accurate a manner as possible. The map was divided to 20 minutes in R.A. and 3° in P.D., and the results of the gauges were figured in their proper places, the areas of the spaces were

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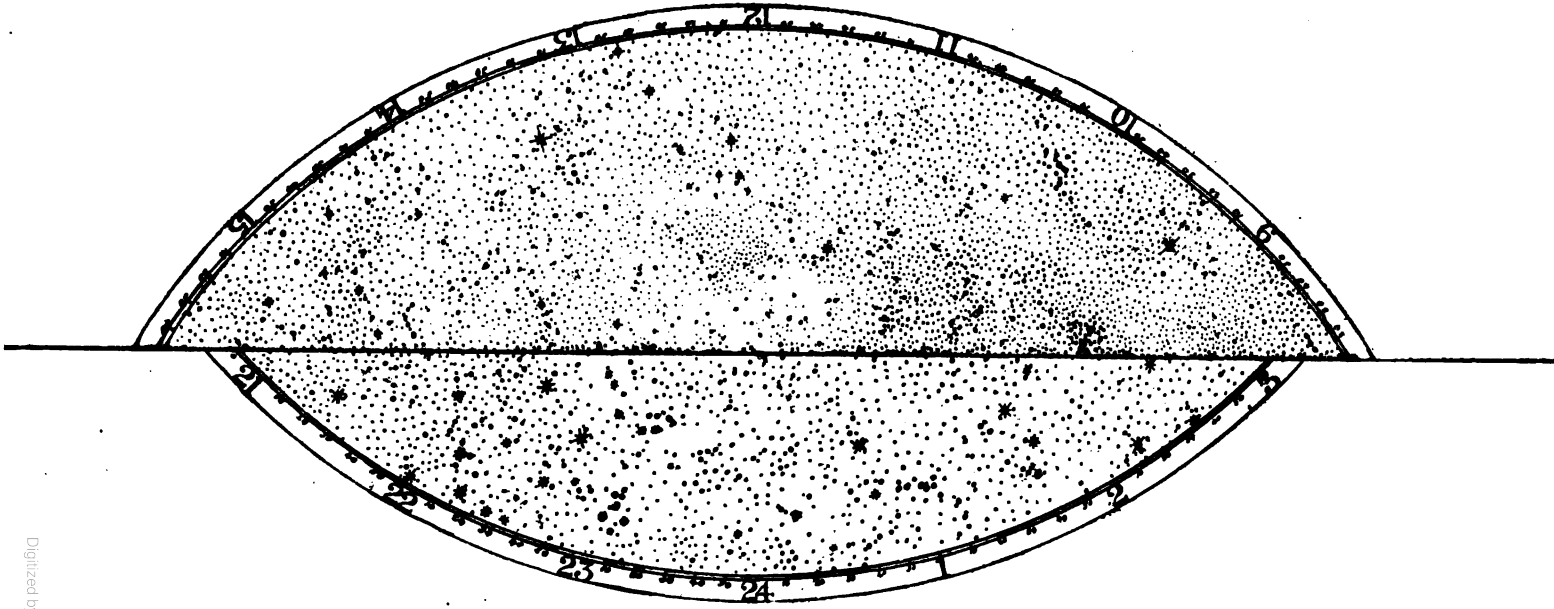

HERSCHEL'S STAR-GAUGES IN THE SOUTHERN HEAVENS

AND THE STARS VISIBLE TO THE NAKED EYE

Drawn on the Isographic Projection

BY SIDNEY WATERS F.R.A.S.

THE POSITIONS OF THE STARS VISIBLE TO THE NAKED EYE ARE FROM MR PROCTOR'S CHART.

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calculated, and the average of the gauges falling on each space multiplied by its area, the result being dotted as nearly as possible equally over the space. Nearly all the spaces were covered in this way; but in those cases where no gauges fell the average of the surrounding spaces was taken. It will be understood that the number of stars marked in the map is very small in comparison with the number which actually exist, but they represent with fair accuracy the state of distribution over the southern heavens down to the faintest stars visible in Sir John Herschel's 18¼-inch Reflector. The stars visible to the naked eye were afterwards added.

The teaching of the chart does not confirm the results of Mr. Stone's investigations published in the *Monthly Notices* of last March. The great massing of stars in parts of the Milky Way, its complicated form, and the sudden decrease of stars at its borders are facts not consistent with the view that we are merely looking through an immense depth of stars, or that distance is the principal reason of their faintness. It is not denied that brightness is an important factor in the determination of stellar distances; but the point contended for is, that it is not a sufficient basis upon which to found any theory of the construction of the heavens.

With regard to the list of star parallaxes which Mr. Stone gives, he states that "we have here a proved range of stellar distance in at least the proportion of 1 to 10." The weight of this illustration will be properly estimated when it is remembered that both the first and the last on the list (*α Centauri* and *Capella*) are stars of the first magnitude, and that therefore it may with equal force be said that we have a proved range of dimension in at least the same proportion. Again, with regard to Mr. Stone's comparison of Argelander's numbers with the numbers computed on the assumption that the stars are distributed with an approximate uniformity in space, there is an excess in the observed number of ninth magnitude stars of 40,000 over the requirements of the theory, the numbers being 142,000 and 102,780. But it is necessary to take into account the fact that Argelander's number is considerably below the truth, owing to the omissions which inevitably occur in counting faint stars. The discrepancy will therefore have to be considerably increased. Notwithstanding this, Mr. Stone considers that it may be altogether disregarded. I do not think that it will be so readily conceded that a difference of at least one-third of the number involved may be disposed of so lightly.

The points, therefore, that I have to urge are, that the fainter stars increase at a far greater rate than would be the case if Mr. Stone's view were correct, and that the theory that the stars are distributed with any uniformity, either as to space or dimension, is not in accordance with the teaching of the chart which I now beg to lay before the Society.

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Ephemeris for Determining the Positions of the Satellites of Uranus, 1878.

By A. Marth, Esq.

0h G.M.T. 1878.		Ariel. long.	diff.	Umbriel. long.	diff.	Titania. long.	diff.	Oberon. long.	diff.
		°	°	°	°	°	°	°	°
Jan.	25	103·99		358·25		353·63		288·69	
			714·17		434·33		206·73		133·67
	30	98·16		72·58		200·36		62·36	
			·16		·32		·73		·67
Feb.	4	92·32		146·90		47·09		196·03	
			·15		·32		·72		·67
	9	86·47		221·22		253·81		329·70	
			·15		·31		·73		·66
	14	80·62		295·53		100·54		103·36	
			·14		·31		·72		·66
	19	74·76		9·84		307·26		237·02	
			·14		·31		·72		·66
	24	68·90		84·15		153·98		10·68	
			·13		·30		·72		·66
Mar.	1	63·03		158·45		0·70		144·34	
			·13		·30		·71		·67
	6	57·16		232·75		207·41		278·01	
			·12		·30		·72		·66
	11	51·28		307·05		54·13		51·67	
			·12		·30		·72		·66
	16	45·40		21·35		260·85		185·33	
			·12		·29		·72		·67
	21	39·52		95·64		107·57		319·00	
			·11		·30		·72		·66
	26	33·63		169·94		314·29		92·66	
			·11		·29		·73		·67
	31	27·74		244·23		161·02		226·33	
			·11		·30		·72		·67
Apr.	5	21·85		318·53		7·74		0·00	
			·11		·30		·73		·67
	10	15·96		32·83		214·47		133·67	
			·11		·29		·73		·68
	15	10·07		107·12		61·20		267·35	
			·11		·30		·73		·68
	20	4·18		181·42		267·93		41·03	
			·11		·30		·73		·68
	25	358·29		255·72		114·66		174·71	
			·11		·31		·74		·68
	30	352·40		330·03		321·40		308·39	
			714·11		434·30		206·74		133·69
May	5	346·51		44·33		168·14		82·08	

These longitudes of the satellites are reckoned in their orbits from the points where they are at their greatest northern elongations. Their position-angles p_o at these points, and the logarithms of the semi-axes a and b of the apparent orbits are the following:—

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0^h G.M.T. 1878.		p_o	Ariel. log. a	Ariel. log. b	Umbriel. log. a	Umbriel. log. b	Titania. log. a	Titania. log. b	Oberon. log. a	Oberon. log. b.
Jan.	25	8·13	1·1812	0·6528	1·3252	0·7968	1·5401	1·0117	1·6663	1·1379
	30	8·19	·1819	·6584	·3259	·8024	·5408	·0173	·6670	·1435
Feb.	4	8·24	·1825	·6639	·3265	·8079	·5414	·0228	·6676	·1490
	9	8·30	·1829	·6693	·3269	·8133	·5418	·0282	·6680	·1544
	14	8·35	·1831	·6746	·3271	·8186	·5420	·0335	·6682	·1597
	19	8·41	·1831	·6796	·3271	·8236	·5420	·0385	·6682	·1647
	24	8·47	·1829	·6843	·3269	·8283	·5418	·0432	·6680	·1694
Mar.	1	8·53	·1825	·6887	·3265	·8327	·5414	·0476	·6676	·1738
	6	8·59	·1819	·6927	·3259	·8367	·5408	·0516	·6670	·1778
	11	8·64	·1812	·6963	·3252	·8403	·5401	·0552	·6663	·1814
	16	8·69	·1803	·6994	·3243	·8434	·5392	·0583	·6654	·1845
	21	8·74	·1792	·7021	·3232	·8461	·5381	·0610	·6643	·1872
	26	8·79	·1780	·7042	·3220	·8482	·5369	·0631	·6631	·1893
	31	8·83	·1766	·7059	·3206	·8499	·5355	·0648	·6617	·1910
Apr.	5	8·86	·1751	·7070	·3191	·8510	·5340	·0659	·6602	·1921
	10	8·89	·1735	·7076	·3175	·8516	·5324	·0665	·6586	·1927
	15	8·91	·1717	·7077	·3157	·8517	·5306	·0666	·6568	·1928
	20	8·93	·1699	·7072	·3139	·8512	·5288	·0661	·6550	·1923
	25	8·95	·1681	·7063	·3121	·8503	·5270	·0652	·6532	·1914
	30	8·95	·1662	·7048	·3102	·8488	·5251	·0637	·6513	·1899
May	5	8·95	1·1642	0·7029	1·3082	0·8469	1·5231	1·0618	1·6493	1·1880

These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then found by means of the equations

$$s \sin (p_o - p) = b \sin \text{long.}$$
$$s \cos (p_o - p) = a \cos \text{long.}$$

Note on the Satellites of Mars and Saturn.

By A. A. Common, Esq.

The following observations of the satellite of *Mars* were made with an 18-inch silvered glass reflector, with powers 170 and 272. In all cases the full aperture was used under favourable atmospheric conditions. The satellite was always fairly visible when the planet was hidden behind a bar in the field. On the 15th and 16th of September it was seen well, with the planet, at times. The difficulty of getting measures of position under these circumstances was got over by the use of a modification of the old slipping piece, which is to be recommended for similar observations.

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This modification consists in placing the slipping piece on the position-circle, and the micrometer on that; thus the movements of the slipping piece are always, after adjustment, parallel to the horizontal and vertical wires of the micrometer.

The vertical wires of the double micrometer were taken out and two thin bands of steel put in (leaving the position or horizontal wire in). In using this arrangement, the zero being found, the position wire is placed upon or beside the satellite, with the planet hidden by the bars; then, by giving to the micrometer the motion parallel to this wire, the place of the wire with regard to the centre of the planet can be seen without altering its relation to the satellite, and, by alternately looking at both, a fair measure may be got; always subject to the error of estimation of the centre of the planet.

The colour of the satellite was distinctly the same as that of the brighter portions of the body of the planet, and had not the bright appearance of the inner satellites of *Saturn*.

It was looked for while nearer (visibly) to the planet, but not seen.

On the 15th of September, at 11^h, while looking at the satellite, a very faint point was seen about 20″ north, very much fainter than the satellite; in fact, barely visible in the glare. This was found next evening to be a very small star, about 120° position-angle and 65″ or 70″ distant from a pair of about 9th magnitude stars preceding the planet (on the 15th).

The inner satellite was looked for, but possibly only glimpsed on October 17, when the position angle was 232° 30′, and dis. est. 15″.

The observations of the satellites of *Saturn* were made with the same instrument. Reducing the aperture brought out details on the body of the planet, particularly on October 26, when the shadow of the planet was well seen on the north following portion of the ring, extending nearly to the width of the space between the limb and the inner edge of the ring. *Hyperion* was frequently seen, though only once measured. *Enceladus* could not be seen when very near the edge of the ring. *Mimas* was never distinctly seen, though frequently looked for. The shadow of *Titan* was seen on October 22, at 9.55, when it had passed central conjunction by about 5″, and on November 23, when it had just got on at 5.45, clouds preventing complete observations :—

Satellite of Mars.

Date 1877.	G.M.T. h m	Dis.	Pos. ° ′	Value 1 to 5.
Sept. 11	9 52		270	Est.
15	10 15		253 48	1
,,	10 20		252 44	1
,,	10 41		251 2	2
,,	10 47		250	1

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Date 1877.	G.M.T. h	m	Dis.	Pos. °	′	Value 1 to 5.	
Sept. 15	10	50		250	40	1	
,,	12	43		244	28	1	
,,	12	51		240	40	3	
,,	13	5		243		1	
16	12	43		273		1	
,,	12	47		269		1	
,,	12	50		273	30	3	
,,	12	57		270		3	
,,	13	5	68″·2			3	
,,	13	44		266	4	2	
,,	13	50	72″·1			3	
,,	13	58		263	4	2	
,,	14	0		263	36	3	
,,	14	5	75″·6	264	8	2	
21	8	34		62	8	1	
24	8	49		240	30	3	
,,	9	8		236	30	1	
28	11	23		111		1	
29	9	28		244	30	2	
,,	9	33		245	45	3	
,,	9	38		235	35	3	with N. Limb.
,,	9	42		251	15	3	with S. Limb.
,,	9	45		254	0	3	with S. Limb.
,,	9	48		232	30	3	with N. Limb.
,,	10	29		242		2	
,,	10	36		239	3	2	
30	10	28		269	30	3	
,,	10	38		267	30	2	
,,	10	52		267	0	1	
,,	11	0		266		1	
Oct. 16	9	55		67		1	
,,	10	0		67	16	1	

Satellites of Saturn.

1877.	G.M.T. h	m	
Sept. 19	12	35	Conjunction of *Titan* with preceding limb of planet.
Oct. 6	7	53	Inferior conjunction of *Titan* with centre of planet. *Titan* 3″ (est.) south.
11	11	0	*Hyperion*—position 92°, distance 220″.
16	7	0	*Japetus*, about 6″ from conjunction with preceding edge of ring.

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1877.	G.M.T. h	m	
Oct. 16	9	43	Nearly "up" to conjunction.
	10	15	"Up."
	10	20	Certainly "up"; distance from plane of ring 48″·8.
17	7	30	*Japetus* about half-way between limb and following edge of ring.
	8	45	*Japetus* 41″·6 distance from plane of ring.
	9	35	Nearly "up"—observation stopped; conjunction would not take place later than $9^h\ 55^m$.
Nov. 25	7	5	*Japetus* 10″ past conjunction with centre of *Saturn*.

Eaton Rise, Ealing,
January 1878.

Note on the Transit of the Shadow of Titan over Saturn, on 1877, *December* 25. By Lord Lindsay.

Dr. R. Copeland observing with the 15-inch refractor (reduced to 10-inch), power about 200.

h	m		
5	36	G.M.T.	Certainly not up.
	42		" "
	44		" "
	46		Not up.
	48		Barely up.
	50		Up.
	53		Still apparently central.
	55		" "
5	56·5		Scarcely past.
6	0·0		Very slightly past (very good definition).
6	2		Very slightly past.
6	5		Certainly past.
6	8		" "
6	12		Far past.

The following pairs may be combined :—

h	m		h	m		h	m	
5	46	Not up.	5	48	Barely up.	5	50	Up.
6	5	Cert. past.	6	0	Very slightly past.	5	56·5	Scarcely past.
5	55·5		5	54		5	53·3	

Say central passage $5^h\ 54^m{\cdot}3$ with limits not exceeding $5^h\ 48^m$ to $6^h\ 0^m$.

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On the whole the definition was very bad. The following measures were taken with great difficulty:—

	h	m	″
Shadow south of axis of ring at	5	45	3·34
		47	3·47
	5	52	4·50
	6	1	2·15
		7	3·43
		14	2·91
	6	17	2·78
Means	6	0·4	3·23

Mr. Lohse observing with the 6-inch Simms made the shadow

	h	m	
Certainly not up at	5	43	G.M.T.
Certainly past at	6	2½	
and Central at	5	53,	

definition very unsatisfactory.

The Observatory, Dun Echt, Aberdeen, 1877, Dec. 26.

On a Cyclonic Arrangement of the Solar Granules.

By W. Huggins, D.C.L., LL.D., F.R.S.

The splendid solar photographs which are taken by Dr. Janssen at Meudon, one of which he has kindly presented to this Society, shows, among other important features, a frequent

FIG. 1.

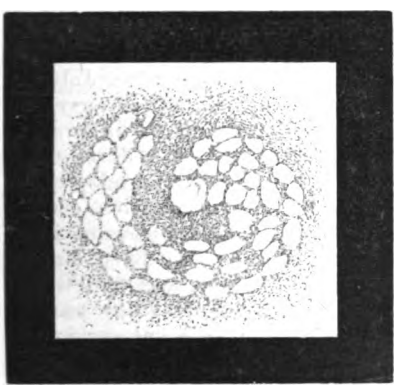
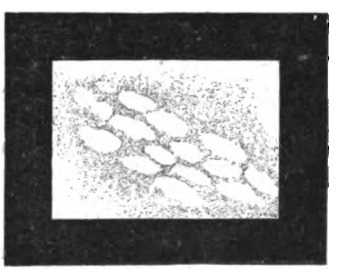


FIG. 2.



tendency of the granules to arrange themselves in a spiral form, accompanied by more or less loss of distinctness of outline of the individual granules, an appearance which suggests great

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cyclonic disturbances of the part of the solar atmosphere in which these bodies lie. It may be of interest, perhaps, to put on record a similar appearance which was observed in 1866.

The Notes made at the time read thus:—

"Saw distinctly the granules. A spiral band of closely associated granules, ending in one of larger size (fig. 1). In one area near the centre of the Sun's disk, the granules appeared more elongated than usual (fig. 2), rather sparsely scattered, and the larger diameters very nearly in the same direction. In neighbouring areas the granules smaller and less elongated. Amongst these no general direction was observed."

On the Probable Errors of Transits of the First and Second Limbs of the Sun, observed by the Chronographic Method. By A. W. Downing, B.A. (Dublin).

It has always appeared to me, and I believe the impression prevails amongst most persons who are in the habit of constantly observing transits of the Sun, that the transit of the first limb is a more difficult observation than that of the second. This difficulty is occasioned, no doubt, by the fact, that in the case of the first limb the wires of the transit instrument are rendered faint by the glare of the advancing limb, whilst in the case of the second limb they appear as dark lines projected on the bright disk of the Sun as a background, and can be watched with ease during the transit of the limb. It occurred to me then, bearing this circumstance in mind, that it would be a matter of some interest to ascertain if the probable errors of transits of the Sun's first limb were sensibly greater than those of the second, and I accordingly entered upon the investigation, the results of which I have now the honour of laying before the Society. The materials used in the computation were the observations of the Sun made at Greenwich with the transit-circle from 1874, July, to the end of 1875, 100 complete transits having been observed during that time, and the mode of reduction being as follows: The time of transit over each separate wire has been reduced to the central wire by applying to the recorded time the adopted equatoreal interval between the wire and the central wire, corrected for Sun's declination and increase of right ascension in the interval, and thus for each complete transit we have nine "central wires." I then take the mean of these, and find the difference of each from the mean. This being done for each complete observation, we have a number of "apparent errors," from which the probable error is found by the usual formula.

The following Table exhibits the resulting probable errors found by this process, and I have added, for comparison, the results for the transit of a star, given by Mr. Dunkin in the *Monthly Notices*, vol. xxiv., p. 154:—

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Object.	Probable Error of a Transit over one wire.	Probable Error of a complete Transit.
	s	s
⊙ 1 L.	± 0·072	± 0·024
⊙ 2 L.	± 0·063	± 0·021
⊙ Centre	± 0·048	± 0·016
A Star	± 0·051	± 0·017

It appears from this Table that the probable errors of transit of the first limb is slightly greater than that of the second; both being, as might be expected, somewhat greater than that of a star.

It must be remarked that the numbers given above comprehend not only the errors of observation, but also those of the adopted equatoreal intervals of the wires and the errors arising from the operation of translating the punctures on the chronograph-barrels into figures; but these may safely be assumed to affect equally the observations of the first and second limbs, and so do not influence the relative magnitudes of the resulting probable errors.

I have, next treated separately the observations made by each person, and so found five results, one for each of the four regular observers, and one for a miscellaneous group of junior observers, composed chiefly of supernumerary computers. The observations are perhaps not sufficiently numerous to determine with accuracy the probable errors of each observer's transits, but they enable us to see the general tendency. The results are given in the following Table and are compared, as far as possible, with the corresponding values for a star, as in the first Table:

Object.	Probable Error of a Transit over one Wire.				
	Ellis.	Lynn.	Criswick.	Downing.	Junior Observers.
	s	s	s	s	s
⊙ 1 L.	± 0·059	± 0·059	± 0·051	± 0·061	± 0·086
⊙ 2 L.	± 0·050	± 0·058	± 0·058	± 0·061	± 0·068
⊙ Centre	± 0·039	± 0·041	± 0·039	± 0·043	± 0·055
A Star	± 0·053	± 0·052	± 0·048	...	...

It is thus seen—confirming the opinion that the transit of the first limb is the more difficult observation—that the larger probable error found in the general result for the transit of the Sun's first limb is mainly due to the observations made by the junior observers, the more experienced assistants observing both limbs with nearly equal accuracy; in fact, the general result for the four senior observers is practically identical for both limbs, viz. for the first $\pm 0^s{\cdot}057$, and for the second $\pm 0^s{\cdot}058$, for a transit over one wire.

We may conclude, then, that for observers of experience the mean results of transits of the Sun observed by the chronographic method will be equally accurate for both limbs; but that for persons who are only occasionally employed in observing transits there will be a tendency to observe the first limb with less

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accuracy than the second, the transit of the first limb being the more difficult phenomenon to observe, and requiring care and practice for its accurate observation. The only account of a previous investigation of this nature that I know of is to be found in the *Proceedings of the Royal Irish Academy*, vol. vii. p. 371, where Dr. Robinson states that, from his observations made at Armagh in 1830, he found for the probable error of transit of Sun's first limb $\pm 0^s.116$ and for that of second limb $\pm 0^s.087$, whilst stars gave the result $\pm 0^s.097$ (these observations, of course, were made by the "eye and ear" method); but I can discover no further details of this computation.

As an *addendum* to this paper, though not immediately connected with the subject of it, I may mention that I find that Argelander states (*Vierteljahrsschrift der Astron. Gesellschaft*, 1872, p. 16) that a transit over the first wire of a set is not as accurate as that over succeeding wires; and as I had the materials at hand, I thought I might as well test the statement in the case of the transits of the Sun observed at Greenwich. I have accordingly computed the probable errors of transits of the Sun's limb over wire i. and over wire ix. of the system of nine wires used for the observation of transits by the galvanic method, with the following result:—

	Probable Error of a Transit over Wire i.	Probable Error of a Transit over Wire ix.
	s	s
⊙ 1 L.	± 0·075	± 0·073
⊙ 2 L.	± 0·071	± 0·070,

thus showing a slightly smaller probable error for wire ix.; but I know not whether it is decided enough to confirm Argelander's opinion, or ought to be considered as merely accidental.

On the Evidence of Periodic Variability of the Nebula H. II. 278, 1860·0, $\alpha = 2^h\ 23^m\ 25^s$, $\delta = -1^\circ\ 43'.0$. By Prof. Winnecke.

Astronomers are unanimous, with regard to the variability of nebulæ, only about the variability of *one* Nebula, the very interesting object discovered by Mr. Hind, 1852, October 11, in $\alpha = 4^h\ 13^m\ 48^s$, $\delta = +19^\circ\ 11'.2$ for 1860, which has been easily observed with the annular micrometer of a $4\frac{1}{2}$-inch refractor by D'Arrest in the years 1855 and 1856. This Nebula has now completely vanished; it is not visible at present in our most powerful telescopes.

During the construction of the University Observatory at Strasburg, and in expectation of the 20-inch refractor* by Merz, mounted by Repsold, which is intended to be used for a thorough

* Merz has finished two very good object-glasses of 20-inch free aperture; one of them has been purchased, after severe trial by myself, for Strasburg in September last.

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revision of the nebulæ, I commenced working, more than two years ago, in this field of research with an orbit-sweeper of 6½-inch aperture* (object-glass by Reinfelder and Hertel of Munich, mounting and micrometer by Repsold of Hamburg), and I have now micrometrical measures of more than 300 nebulæ, with the stars in their vicinity, generally on more than one night.

In comparing the former measures and descriptions with my own, strange differences are not seldom noticed; one of the most curious instances is presented by the Nebula H. II. 278 (1860·0, $\alpha = 2^h\ 23^m\ 25^s$, $\delta = -1^\circ\ 43'{\cdot}0$). I find the following observations:—

H.	1875	Jan. 6	*p*B. S.E.; 1 obs.
h.	1827	Nov. 24	*p*B. S.E.; *b*M.
h.	1827	Dec. 20	*p*5*b*M.
R.	1856	(Nov. 29?)	Nothing particular.
D'Arrest	1856	Jan. 2	Nicht klein, doch sehr schwach. (Refractor by Merz, 4½-in. ap.)
Schönfeld	1861	Dec. 2	Ich habe, 1861, Dec. 2, bei Luft I mich nicht sicher von seiner Existenz überzeugen können, sondern nur notirt: *Vielleicht gesehen.* Die Sichtbarkeit ist allerdings vielleicht durch den Stern 75 *Ceti* (5ᵐ·6), der nördlich folgt, etwas erschwert. (Refractor by Steinheil, 6½-in. ap.)
D'Arrest	1863	Nov. 3	Non egregie serenum. Subclara, minuta; manifesto haud "majuscula" ut in observatis meis Lipsiensibus annotatum reperio. Nucleum habet fixæ 13ᵐ haud dissimilem. Ipsa nebula oblonga, et quidem notabiliter. * 13 ord. proxime seq. ad austrum.—Turbulenter omnia: densa nubes eripuit nebulam. (Refractor by Merz, 11½-in. ap.)
D'Arrest	1863	Nov. 7	Sudum conspicua, ellipsis satis stricta. Nucleum monstrat fixæ 11ᵐ·12 similem. 12 ord. seq. 5·75ˢ 2′ vel 3′ ad meridiem.
Schönfeld	1864	Feb. 2	Luft 1–2. Mässig schwach. Scheint nicht rund. 5*b*M. Kernlicht = 12ᵐ·2, aber nicht sternartig. 12ᵐ seq. A dist. 2½′. (Steinheil, 6½-in. ap.)
D'Arrest	1864	Nov. 6	Apprime serenum. Clara, longula; nucleus = * 13 mag. * 12ᵐ mag. seq. 5ˢ·6 nonnisi 1¾′ ad meridiem.
Vogel	1865	Nov. 13	Bei äusserst durchsichtiger Luft vergeblich gesucht, desgleichen Nov. 20. (Steinheil, 8½-in. ap.)
Schönfeld	1868	Jan. 16	Luft 2–3, 6″, rund, Kern fast sternartig 11ᵐ·12, 11ᵐ seq. A dist. 3′.

* For a notice on the beginning of these observations and a description of the orbit-sweeper and its micrometers, see *Vierteljahrsschrift der Astron. Gesellschaft*, Band x. pp. 297–305.

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Winnecke	1877	Jan. 6	Gut II Klasse, 1¼′ gross mit ... 1. Mit 170 facher Vergr. 30″ l., 20″ br., verlängert in $p = 7°{\cdot}2$.
Winnecke	1877	Jan. 10	Ein recht heller Nebel, II Klasse, 30″ l., 20″ br., zur Mitte stark, fast fixsternartig verdichtet.
Winnecke	1877	Sept. 14	Obgleich der Himmel sich wieder ziemlich stark mit Cirrus bezogen hat, sehr gut sichtbar, 1′ lang, gedehnt in $p = 20°$; scheint einen fixsternartigen Kern $= 12^m{\cdot}13$ zu haben.

The descriptions of the Nebula by the Herschels agree entirely; nor has Lord Rosse noticed anything particular. But in December 1861 the Nebula, which had been observed five years before by D'Arrest with a much smaller refractor, was *invisible* to Schönfeld with the splendid Mannheim refractor. In the years 1863 and 1864 it is observed both by Schönfeld and D'Arrest; 1865, November 13 and November 20, it is *again invisible* to the sharp eyes of Dr. Vogel with the Leipzig refractor. It may be noticed that an error in pointing the telescope on these days is impossible, for Dr. Vogel has observed other nebulæ near it on November 13 and 20. Schönfeld observed the Nebula again in the year 1868 without difficulty, and in the present year it is a *very easy* object for 6½-inch aperture.

It seems, therefore, probable that the quantity of light from this object reaching our eyes is very different in different years; a most important conclusion, as it is the first instance of a very great evidence for *periodic* variability of a Nebula.

Strasburg, 1877, *Dec.* 11.

On a Cause for the Appearance of Bright Lines in the Spectra of Irresolvable Star Clusters. By E. J. Stone, M.A. Camb., Her Majesty's Astronomer, Cape of Good Hope.

In the Supplementary Number of the *Notices*, vol. xxxvii., there appears a short paper by Mr. Proctor "On the Arc of Light seen round *Venus* in transit." To this paper are appended some remarks which are, I presume, intended to be a criticism of my paper which appeared in the *Proceedings of the Royal Society*, No. 180, 1877. It appears to me, however, that Mr. Proctor has missed the point of my paper; and as I should prefer that my meaning be understood, I shall give Mr. Proctor's remarks and a statement of my views, extracted from the *Proceedings of the Royal Society*, for comparison with each other.

But before proceeding to this point I may perhaps be allowed to mention that Mr. Proctor's paper "On the Arc of Light seen round *Venus* in transit" is a particular case of the problem investigated by Sir G. B. Airy in the *Monthly Notices*, No. 1, vol. xxiv., 1863. Sir G. B. Airy proves that, if a part of the Sun's disk is viewed through a refracting medium which produces

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an effect analogous to that of a convex lens, that part of the disk will not be of greater apparent brightness than the rest; or, "that no refraction can cause a change in the apparent brightness of the surface visible." As Prof. Airy's investigation is very complete and covers the ground of Mr. Proctor's paper, I presume that Mr. Proctor must have been unacquainted with the investigation at the time his paper was written.

The following are Mr. Proctor's remarks upon my paper:—

"It is asserted that an irresolvable stellar nebula would diminish in apparent brightness (so far as the stellar part of its light is concerned) with increase of distance. No reasoning is given in support of this assertion, and I can see no reason for withdrawing the reasoning by which, in a paper 'On the Resolvability of Nebulæ regarded as a test of distance,' I show (incidentally) that a nebula, so long as it remained irresolvable, would be of constant intrinsic brightness whatever its distance."

My statement of my views is as follows:—

"The Sun is known to be surrounded by a gaseous envelope of very considerable extent. Similar envelopes must surround the stars generally. Conceive a close stellar cluster. Each star, if isolated, would be surrounded by its own gaseous envelope. These gaseous envelopes might, in the case of a cluster, form over the whole or a part of the cluster a continuous mass of gas. So long as such a cluster was within a certain distance from us, the light from the stellar masses would predominate over that of the gaseous envelopes. The spectrum would therefore be an ordinary stellar spectrum. Suppose such a cluster to be removed farther and farther from us. The light from each star would be diminished in the proportion of the inverse square of the distance; but such would not be the case with the light from the enveloping surface formed by the gaseous envelopes. The light from this envelope received on a slit in the focus of an object-glass would be sensibly constant, because the contributing area would be increased in the same proportion that the light from each part is diminished. The result would be, that at some definite distance, and all greater distances, the preponderating light received from such a cluster would be derived from the gaseous envelopes, and not from the isolated stellar masses. The spectrum of the cluster would therefore become a linear one, like that from the gaseous surroundings of our own Sun. The linear spectrum might, of course, under certain circumstances, be seen mixed up with a feeble continuous spectrum from the light of the stars themselves.

"It should be noticed that, in this view of the subject, the linear spectrum can only generally appear when the resolvability of the cluster is at least injuriously affected by the light of the gaseous envelopes becoming sensibly proportional to that from the stellar masses, and that in the great majority of such cases it would only be in the light from the irresolvable portions of the cluster that bright lines could be seen in the spectrum."

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When the paper was read, it appears that Professor Stokes objected that, although the contributing area of the gas would be increased in the way indicated, yet other stellar masses would at the same time be crowded into the line of sight, and that it need not therefore follow that the spectrum should become linear with increase of distance.

So long as the gaseous envelopes are considered to be isolated round each individual star, and to be of comparatively speaking small dimensions, the objection is a valid one; but the clusters which I had under contemplation were physical clusters, and this isolation of the gaseous surroundings would be, under suitable conditions, destroyed by the relative gravitation and heating effects of the several members of the system. It is to such a cluster only that my reasoning applies; and I think such will be seen to be the case if my paper is carefully read.

I need hardly say that, after I heard of Professor Stokes' objections, I carefully reconsidered the theory; but after the best consideration I can give the subject, I believe my conclusion is sound, and there the matter must rest for the present so far as I am concerned.

With respect to Mr. Proctor's own views: If he thinks that he has proved that, because a stellar cluster is irresolvable with a telescope of small aperture and small power, it must follow that "it would be of constant intrinsic brightness at all greater distances," I think there would be no great difficulty in proving that he is mistaken. I have introduced the words "small aperture" and "small power" as presenting the case in an exaggerated form, and merely to indicate the point upon which I think his theory fails.

I must also, as bearing on the same point, call attention to a very general statement of Mr. Proctor's, that "he has shown that by no optical contrivance can apparent brightness be increased, though quantity of light can of course be greatly increased." This statement certainly requires qualification; for, if it were true, why could not the satellites of *Mars* be as easily seen with a 4-inch as with the 26-inch of the Washington Observatory? I know, of course, what Mr. Proctor should, and probably does, mean; and I should not have called attention to this point had it not been the fact that the principal cases of exception are those which have been under consideration in the present paper.

Note on Specular Reflexion from Venus.

By W. H. M. Christie, Esq.

In the *Monthly Notices* for December 1876, I gave some observations on the gradation of light on the disk of *Venus*, made with the object of testing Mr. Brett's hypothesis of specular reflexion

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modified by atmospheric diffusion. The position of the brightest point of the disk, determined with all possible care, agreed with that assigned by this hypothesis, whilst it was incompatible with any theory of reflexion from an unpolished surface. The disk was then somewhat gibbous, and the whole question turned on the estimate of position, about which, however, I feel pretty confident. I have now made some observations of *Venus* in the crescent form with the Greenwich Equatoreal, the result being entirely to confirm Mr. Brett's original statement, that the brightest part of the crescent is a little within the limb. On December 31, 1877, I found, on reducing the intensity of light by means of the polarising eye-piece previously referred to, that the breadth of the crescent diminished from about 0·8 of the angular radius of the disk to 0·2, fading off on both sides as well as towards the cusps; and in the *middle* of the breadth of this narrow strip I was able to detect a very fine line, the length of which I estimated at about one-fourth of the radius. This observation was made in the evening between 5^h and 6^h. Allowing for the loss of a small portion on the side of the limb (when the light was reduced to one 3000th part), of which I satisfied myself by repeated comparisons, I estimated this brightest portion of the disk to be 0·2 of the radius within the limb. The calculated position on the hypothesis of specular reflexion, as afterwards found, would be 0·22 of the radius from the limb. On 1878, January 9 and 10, I had an opportunity of examining *Venus* in the daytime, and was fortunate in securing Capt. Tupman's co-operation on these occasions. As seen with an ordinary eye-piece (power 500) *Venus* appeared as a crescent, having a breadth equal to about two-thirds of the radius, with a bright arc near the limb. This arc shaded off perceptibly towards the cusps. On applying the solar eye-piece, which reduces the light (by virtue of the three reflexions from glass) to one 3000th part, both Capt. Tupman and I satisfied ourselves that there was a bright elongated patch distinctly within the limb. In the centre of this I caught glimpses of a small point of light, the distance of which form the *apparent* limb I estimated as rather more than 0·1 of the radius. The position of the point at which specular reflexion would take place, I afterwards found by calculation to be 0·18 of the radius within the limb.

Although the estimates of position of the bright point are, from the nature of the case, rough, the agreement with the specular reflexion hypothesis is very striking; and these observations would establish the fact that there is a gradation of light on the disk of *Venus*, which can only be explained on that theory. On the supposition of reflexion from a rough or unpolished surface, there would, according to Mr. Neison, be a point of maximum illumination, but it would fall on the farther side of the limb, and as the limb itself, at the time of the second observation (January 9), was distant 20° of longitude from this point, the increase of brightness would be very marked right up to the limb. Now the actual circumstances are very different. There

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is a portion of the disk near the limb which at a first glance appears to be uniformly brighter than the part near the terminator. The breadth of this bright arc was, on January 9, about one-fourth of the radius, so that at its inner edge the Sun's zenith distance must have been 60°, whilst at the limb it would be 20°. Thus, if we had to do with reflexion from a rough surface, there ought to be a decided gradation up to the very limb, the brightness at that part being, on this supposition, twice as great as at the inner edge of the bright arc. This is emphatically not the case.

The bright patch here referred to was described by Capt. Noble in the *Monthly Notices*, June 1876, when *Venus* was under similar conditions of illumination, and his failure to detect any gradation whatever towards the limb appears conclusive against the theory of reflexion from a rough surface. Had he looked a little more closely, he would, perhaps, have detected in the middle of the bright patch a small bright point, as I did. At any rate, the existence of a patch of sensible area, and "without the slightest sign, trace, or indication of one part being in the smallest degree brighter than another," seems utterly incompatible with any theory whatever. But on the hypothesis of specular reflexion modified by diffusion in a dense atmosphere, the point of maximum brightness would be in the middle of this patch, and the gradation on either side, being comparatively slight in the neighbourhood of the maximum, might easily be overlooked by a hasty observer. But I believe that, by patient gazing under favourable circumstances, the actual image of the Sun may be seen as a minute speck. A high power, however, must be used, as the speck would hardly exceed 0″·1 in diameter. I have used thoughout a power of 500. The reduction of the light by a dark glass or other means is important, as it removes any disturbing effect of irradiation and enables the eye to appreciate more readily small variations of brightness. It may be well to add, that the position of plane of polarisation in the eye-piece relatively to the line of cusps appeared to have no appreciable effect on the gradation of light. This would be consistent with the theory of reflexion from a metallic surface; but the observation is so delicate, that I would not lay much stress on the circumstance that no trace of polarisation was noticed.

1878, *January* 10.

P.S.—On 1878, January 11, after the above was written, Mr. Maunder examined *Venus* both with the polarising eye-piece and a dark wedge, and noted a bright line or elongated patch, about 7″ or 8″ in length and 2″ in breadth, the middle of which was from 2½″ to 3″ within the limb, i.e. about 0·15 of the radius. As the brightness was still further reduced the ends of the bright line disappeared, and the last phase was a tiny faint blur. These observations were quite independent, and Mr. Maunder had, in fact, no knowledge of my results.

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Suspected Repetition, or Second Outbursts from Radiant Points; and on the Long Duration of Meteor Showers. By W. F. Denning, Esq.

The radiant-points which I have reduced—chiefly from my observations of the paths of 2,690 shooting stars between April 1876 and December 1877, and from the Catalogue of the Italian Meteoric Association (1872), which I have recently been comparing together and with the positions obtained by other observers—appear to prove that many showers have very long duration. In some well-marked cases the period extends over four months at least, which is much greater than that commonly assigned to them; in fact, I believe that, of the showers previously known, not more than three last longer than eight or nine weeks, and certainly none longer than twelve weeks. My own observations and reductions would, however, considerably extend the duration generally, and there appears to be a tendency in many radiants to repeat themselves (or give secondary outbursts), at an interval of about three months after the first display. There is no absolute cessation of action during the intervening lull, though meteors come very sparingly, and seldom in sufficient number to constitute a "shower," on one or two nights. It is true that there are now so many known radiant-points differing in dates, positions, intensities, &c. (and these only approximately ascertained in numerous instances), that it is but fair to expect many accidental coincidences in position such as I have been referring to, and that they are merely different showers coming later from same places. This seems the natural explanation, and it is rendered more probable by the fact that meteor showers make their appearance, as a rule, round about the Earth's apex, and are rarely seen active or well defined far away from it. In fact, these systems are generally confined to a minor part of the heavens, and thus it would be singular indeed if, with so many different showers, we had no instances in which the radiant positions were found to overlap or overlie. There is also this to be said, that absolute accuracy is never attained in this branch of observation, and that a radiant of a feeble system can seldom be relied upon, except in special cases, to within five or seven degrees. Moreover, the idea of these repeating radiants (assuming them to belong to the same parent system), is opposed to the relation of comets and meteors in the simple form we understand it, and cannot easily be accounted for. It seems to me, however, that, notwithstanding all this, the large number of the agreements and their exact nature are strongly suggestive of some connection, and require a different explanation to that indicated above. I have no longer any hesitation in mentioning it as a point deserving special attention from workers in this department. In the present very imperfect state of our knowledge it would be wrong to hastily condemn any observed facts, however improbable and incomprehensible they may at first sight appear.

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The following are a few cases of these radiants which I have selected as samples of many others that might be adduced. The positions are from my own observations or the Italian ones, and a few are included that I reduced from the late Dr. Heis's recently published work, *Resultate der in den* 43 *Jahren* 1833–75, *angestellten Sternschnuppen Beobachtungen* (Munster, 1877), and Zezioli's Catalogue of 8,623 meteors, 1867-70, which had, however, already been partially reduced, the former by Heis himself, and the latter by Schiaparelli. The various radiants determined from these sources are incorporated, and an average taken, but only in cases when they agreed very closely, and unmistakably belonged to identical systems :—

I. *First Display.*

Dates.	No. of Positions.	No. of Meteors.	Radiant-point. R.A. °	Radiant-point. Decl. °
Aug. 6-12	2	61	97	+71
Aug. 6—Sept.	4	63	46	+73
Aug. 6-12	1	21	76	+33
Aug. 6-16	3	61	70	+64
July—Sept.	5	76	62	+48
Aug.—Sept.	5	82	61	+37
Aug.—Sept.	5	41	44	+24
Oct. 2—Dec.	4	53	131	+20
Nov. 22—Dec. 13	2	47	81	+23
February	1	10	132	+67

II. *Second Display.*

Radiant-point. R.A. °	Radiant-point. Decl. °	No. of Meteors.	No. of Positions.	Dates.
101	+73	27	3	Oct. 2—Dec. 2
43	+73	32*	4	Oct. 28—Nov.
77	+31	17	2	Oct. 2—Nov. 13
69	+66	40	5	Nov.—Dec.
62	+48	38	5	Oct.—Nov. 13
61	+36	33	4	Oct.—Dec.
45	+25	65	4	Oct.—Nov.
132	+20	27	3	Feb.—April
83	+22	23*	2	Feb.—Mar. 12
134	+67	32*	2	April—May 15

* New showers requiring confirmation.

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I believe the 10 pairs of radiants in this Table are accurately placed; they are supported by a large number of meteors and (with four exceptions) agree with showers seen by former observers. Many other such agreements as are here presented will be found by consulting the radiant-lists of Greg and Herschel, Heis, Schmidt, Schiaparelli and Zezioli, and Tupman. The following are a few additional ones from my own observations :—

	° °	° °	
Mar. 18—May 17	286−13	284−11	July 6—Aug. 16.
Sept.—Nov.	130+47	130+44	Jan. 4–20.
April 21—May	280+58	284+57	July 16—Aug. 16.
Dec. 10—Jan. 20	221+41	220+40	Mar. 14—April.
Aug. 3–16.	31+18	30+16	Oct. 28—Nov. 13.
Nov.—Dec.	173+ 9	175+10	March.
Nov.—Dec.	182+36	181+34	Feb. 20.
Oct. 2	225+52	221+52	Dec. 12—Jan. 20.
April 16–19	267+21	267+20	Aug. 5—Sept. 7.
May 15	304+ 7	306+ 6	Aug. 3–16.

The number might be greatly extended. Thus, there are apparently recurring showers in the months of July and October from 30°+36°, 47°+45°, 9°+34°, 80°+56°, 6°+52°, and in October and January from 105°+50°, 104°+13°, 153°+42°, 120°+15°, 136°+78°, &c.; and it appears certain that a radiant-point agreeing with that of the August *Perseids* is continued from that month to December! An average of 14 positions gives 45°+57° as the centre of it. Now Mr. Greg's general central radiant for the August *Perseids* is at 44°+56°, which accords almost precisely with the preceding one. The November *Perseids* Mr. Greg places at 46°+55°, and the position was well confirmed by Heis at 44°+56° from 48 meteors seen during the first half of the month. Thus it is evident that the August and November *Perseids* have precisely the *same radiant-point.* Not only Heis, but Greg and Herschel, and Schiaparelli and Zezioli saw the shower well defined in November. From my own observations and those made in Italy, the radiant also appears tolerably active during the first half of September, and Captain Tupman saw it accurately at 42°+55° on the morning of September 5, 1870. These later meteors from *Perseus*, seldom have streaks like those of about August 10, and it is very probable that they belong to quite a distinct system. There is no doubt, however, that after the maximum on August 10 there is still a lingering shower from the same centre, and that it reaches its greatest intensity about three months after the first display, which confirms the periods observed in revivals of other showers.

It is a peculiar thing to say that a person may see, by looking towards the eastern sky (at the constellations of *Perseus*, *Auriga*, and *Camelopardalus*) in the evenings of November, many of the same radiants that were visible in the early mornings of August,

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or that the radiants observable in the mornings of November come into play again in the evenings of February, &c.; but that seems a probable conclusion from numerous observations and reductions. Fortunately the question may soon be settled without waiting for further observations. The Catalogues before mentioned contain hosts of well-observed meteor paths from which it will be possible to confirm or disprove these reappearing meteor systems, though the work will entail much labour, and there will be some difficulty in getting at the radiant-points accurately. I would suggest that, if it is undertaken, the meteors for August 1–15 be first examined, and those presumably belonging to radiants *eastwards of Perseus* (say between 50°–110° R.A. and 20°–80° North Decl.) selected. This may be done by a glance at the places of beginning and ending, and in this way the mass of ordinary *Perseids* may be avoided. A series of similar star charts must be used in the projections, and the radiant-points determined from each, then the positions of the nearly accordant showers averaged. Following the same plan for the first half of November, the two results would admit of fair comparison. There would be some showers in November not seen in August and *vice versâ*, but in many instances, I believe, the positions will be identical, or so nearly accordant as errors of observation and reduction will allow. I shall look forward with much satisfaction to the confirmation of a new August shower in *Camelopardalus* at 97°+71°, which I found splendidly defined from two independent sources, and it is difficult to understand how Heis missed this radiant (though he saw many of its meteors) during the 43 years that he gave great attention to the meteors of August.

Postscript.

Since writing this paper I have partially reinvestigated the positions on which I chiefly rely as proving these repeating meteor showers, and have been projecting the paths of many hundreds of shooting stars recorded during the first half of January, with the view to ascertain whether a number of radiants I observed in the mornings of last October recurred again, as they should according to my previous observations. The result of the examination was very satisfactory in each case, and apparently proved in the most palpable and conclusive manner the suspicion I have ventured to mention in this paper, namely, that there is more than one outburst from radiant-points, and two maxima at intervals of about three months, little more or less. I do not mean to say that they can possibly represent the same parent systems, and that the two displays are simply manifestations of one stream, because that is incredible and unaccountable from existing theories. The subject is very wide and difficult, requiring yet much further assiduous work before we may hope to unravel the vast and complicated network of meteor streams which the Earth annually encounters.

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Remarks on Mr. Denning's Paper. By Captain G. L. Tupman.

The radiant-point of a meteoric shower very seldom indicates the true direction of motion. When the orbital motion of the Earth is transverse to the true direction of motion of the stream, the radiant is displaced to the extent of 35° towards the apex of the Earth's way (a point in the ecliptic approximately 90° of longitude behind the Sun), on the supposition of the greatest velocity that can reasonably be assigned to meteors—that proper to parabolic motion—which for the present it will be sufficient to consider.

All the meteor streams we are certainly cognisant of at the present time can be accounted for by supposing them to belong to the solar system in the same manner as do comets. In the case of certain streams, as the *Leonids* of November 13, and the *Perseids* of August 10–11, the identity of their orbits with those of known comets is too certainly established to need comment, and these may be taken as typical streams belonging to the solar system.

The spreading out of the individuals in directions perpendicular to the general direction of motion necessarily involves a different orbit for each individual, and in proportion as the spreading out becomes more considerable, there will be less similarity of orbit.

The term *stream* or *shower* should properly be applied to the *ensemble* of a number of particles moving approximately in the same orbit in nearly the same periodic time. It is evident that we can have no knowledge whatever except of those streams that actually intersect the orbit of the Earth. There is a special case in which the Earth can remain in a stream for several weeks with a nearly fixed radiant. The orbit must nearly coincide with the plane of the ecliptic, the perihelion distance of the central portion be a little less than unity, and the motion be direct. The position of the radiant would be 90° before the Sun at the middle time.

If the perihelion distance be supposed to diminish, say to ·8, the Earth would pass through the central part of the stream twice at an interval of about 3½ months. At the first passage the radiant would be about 125° before the Sun, at the second about 30°, and therefore only to be observed shortly before sunrise. In all other cases the reappearance of a radiant after an interval indicates a distinct shower.

The results obtained by Mr. Denning after so much labour are probably to be attributed to the method employed in obtaining the radiants. The method was begun by Heis, and used by Greg, Herschel, Schiaparelli, and others, and was indeed the only method available with few observations. The observations of many nights were combined to obtain the apparent radiation, and the duration was supposed to begin and end with the first and last day so employed.

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It is easy to obtain a radiant in any part of the sky, especially near the apex of the Earth's way, by collecting, night after night, meteors whose tracks, carried backwards, pass near the point in question.

I understand that Mr. Denning's coincidences in no degree depend upon previous knowledge; each is an entirely independent deduction, and therefore much more striking and singular.

We are now, probably, in possession of sufficient observations to enable the radiants proper to each day of the year to be deduced separately, and the number of determinations from multiple observations of solitary bright meteors is steadily increasing. These last are the best of all.

At present we are not obliged to resort to the idea of immense streams of meteors existing in space, and brought into collision with us by the motion of the Sun. The radiant-point of such a stream, moreover, would not be stationary many weeks.

Until the durations of fixed radiants be established in a more certain manner than those in the paper before us, we need not endeavour to explain them.

On a Small Term of Long Period in the Mean Motion of the Moon.

By E. Neison, Esq.

The mean motion of the Moon contains a term of long period, with an argument of the form

$$\{[a-29n''+26n']\,t+\epsilon-29\epsilon''+26\epsilon'-\mathrm{E}-\mathrm{A}_o\},$$

where $(at+\epsilon-\mathrm{A}_o)$ denotes the mean anomaly of the Moon, and $'$ and $''$ quantities relating to *Venus* and the Earth respectively. This term has a period of 130 years, and it appeared probable that its coefficient would be sensible, for it was evident that the terms depending on the eccentricities would not suffer the internal mutual destruction which occurs in the case of the analogous *Venus* term discovered by Hansen. The value of the term was calculated, but the relative positions of the perihelia of the Earth and *Venus* render its coefficient only one-fifth of that it would have possessed had it arisen from the planet *Mars*.

It is, however, very desirable that, when the value of such a term has been determined, it should be placed on record in sufficient detail to save the useless labour of a repetition of the work by subsequent investigators in the same field. Moreover, small as the term is, it might well be included in any future lunar tables, for it has a comparatively short period.

Neglecting terms depending on the Moon's longitude and latitude, then the disturbing function may be written (Delaunay, *Conn. des Temps*, Add. p. 8, 1862)

$$\mathrm{R}^{(1)} = m'r^2.\left\{\frac{1}{4\Delta^3}-\frac{3}{2}.\frac{1}{\Delta^3}.\eta_1^2\sin(v'-\mathrm{B}').r'\,[r''\sin(v''-\mathrm{B}')+2r'\sin(v'-\mathrm{B}')]\right\},$$

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the notation being slightly altered to bring it into accordance with my other result. Suppose the terms of the third order of the eccentricities and inclinations to be neglected, except as contained in the function Δ, and replace Δ^{-s} by its general term,

$$\Delta^{-s} = (a')^{-s}.[B_{\frac{s}{2}}]^{i}_{i+k}.\cos\{[in''-(i+k)n']\,t+E\};$$

then the disturbing function becomes

$$R = m'(a')^{-3}r^2\left\{\frac{1}{4}[B_{\frac{3}{2}}]^{i}_{i+k}\cos\{[in''-(i+k)n']\,t+E\}\right.$$
$$\left.+\frac{3}{8}\eta_1^2\left(a^2[B_{\frac{7}{2}}]^{i}_{i+k+2}+2a[B_{\frac{7}{2}}]^{i+1}_{i+k-1}+2a[B_{\frac{7}{2}}]^{i-1}_{i+k+1}\right)\cos\{[in''-(i+k)n']\,t+E\pm 2B_1\}\right\}$$

Giving i and k the values necessary to bring the indices of $[B_{\frac{3}{2}}]$ to the values 26 and 29, then

$$R = m'(a')^{-3}r^2\left\{\frac{1}{4}[B_{\frac{3}{2}}]^{29}_{26}\cos\{[29n''-26n']\,t+E\}\right.$$
$$\left.+\frac{3}{8}\eta_1^2\left(a^2[B_{\frac{7}{2}}]^{29}_{28}+2a[B_{\frac{7}{2}}]^{29}_{27}\right)\cos\{[29n''-26n']\,t+E-2B_1\}\right\}$$

Terms of the fourth order have been neglected, and a represents, as usual, the ratio $\frac{a''}{a'}$.

If (R) denotes the term in brackets, R may be written

$$R = m'(a')^{-3}r^2.(R).\cos\{[29n''-26n']\,t+E_1\}.$$

Replacing r^2 by its value, and writing $m'\frac{a^3}{a'^3}=\sigma m^2$, and

$$\delta n = -3\iint an^2\left(\frac{dR}{d\epsilon}\right)dt^2 = \sigma m^2.\left(\frac{n}{p}\right)^2.3e.(R)\sin\{[a-29n''+26n']\,t+\epsilon-E_1\}.$$

The coefficient of t in the argument is $p=10{,}171''$. Therefore the value of $\sigma m^2\left(\frac{n^2}{p^2}\right)3e$ is $1{,}378''$. The value of $[B_{\frac{3}{2}}]$ can be found in the usual manner, from either Delaunay or Pontecoulant. Substituting the proper values in the expression for δn, it becomes

$$\begin{aligned}
\delta n = {} & +0''{\cdot}005\sin\{[a-29n''+26n']\,t+\epsilon-29\epsilon''+26\epsilon'-A+3\varpi'\}\\
& -0''{\cdot}067\sin\{\quad,,\quad,,\quad,,+2\varpi'-\varpi''\}\\
& +0''{\cdot}201\sin\{\quad,,\quad,,\quad,,+\varpi'-2\varpi''\}\\
& -0''{\cdot}189\sin\{\quad,,\quad,,\quad,,+3\varpi''\}\\
& +0''{\cdot}049\sin\{\quad,,\quad,,\quad,,+\varpi'+2B'\}\\
& -0''{\cdot}122\sin\{\quad,,\quad,,\quad,,+\varpi''+2B'\}\\
= {} & -0''{\cdot}100\sin\{[a-29n''+26n']\,t+\epsilon-29\epsilon''+26\epsilon'-A_0+180^\circ{\cdot}5\}.
\end{aligned}$$

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By taking into consideration higher powers of the inclination, and the ordinary perturbations of the Moon, the coefficient of the term will be raised to nearly $-0''{\cdot}150$.

On a Secular Term in the Mean Motion of the Moon.

By E. Neison, Esq.

The lunar co-ordinates contain a term of long period due to the action of *Mars*, and with the argument

$$\{[A - 15n'' + 28n'''] t + E\},$$

where At denotes the mean motion of the Moon's perigee, and $n''t$ and $n'''t$ the mean motions of the Earth and *Mars*. This term has the very long period of 6,000 years, and is of the 14th order of the eccentricity of *Mars* and the Earth. The eccentricity of *Mars* is, however, so large that the coefficient of this term is comparatively considerable, despite its high order.

This term will introduce a similar term into the mean motion of the Moon, through the means of the solar perturbations; and this term will be multiplied by the square of the lunar perturbation arising from the action of the Sun, but divided by the cube of the small coefficient of the time. As

$$[A - 15n'' + 28n'''] = +197''{\cdot}715,$$

the term will rise immensely, and will have, apparently, a considerable coefficient. Its calculation will be long and laborious, but, unless its terms destroy each other in some unexpected manner, it will repay the labour.

The present note, however, is directed to a single term with this general argument. The term

$$[A - 15n'' + 28n'''] - 9\varpi''' - 5\varpi''$$

has the coefficient of t reduced to zero from the motion of the perihelia of *Mars* and the Earth. It forms, therefore, a secular term in the mean motion of the Moon, and is, I believe, the first term of this kind due to the direct action of the planets which has been made known. It will obviously increase the secular acceleration by a small fraction, for it will appear in the mean motion as multiplying the square of the time.

Professor Newcombe is, I believe, the first who discovered that the mean motion of the Moon contains terms depending on the motion of the Moon's perigee, a result which I independently found two years back.

I know of at least two similar secular terms in the Moon's mean motion, but they will be much smaller, although they contain the Moon's mean motion in their arguments, for they depend on terms involving products of the fifth order of the lunar eccentricity and inclination.

The existence of these curious terms seemed of sufficient interest to merit their existence being announced.

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On the Correction of the Elements of Ceres.

By William Godward, Esq.

The Ephemerides of Ceres in the *Nautical Almanacs* from 1857 to 1881 have been derived from Schubert's Elements, published in No. 69 of Gould's *Astronomical Journal* (Cambridge, U. S., 1854, April 3). These elements were founded upon 14 normal places 1832-1853, taking account of perturbations by *Jupiter* only, the differential coefficients of which were furnished by Professors Encke and Wolfers, from the calculations for the *Berliner Jahrbuch*. With the corrections given by the solution of equations of condition for the times of the 14 normals, Schubert computed the corrected elements for 1854, January 0, Washington mean time, which are the foundation of the *Nautical Almanac* calculations to 1881.

From these elements, with the addition of perturbations by *Venus*, the Earth, *Mars*, *Jupiter*, and *Saturn*, approximately perturbed elements have been computed for periods of 2,412 days at intervals of 36 days, and used for obtaining the variations of the elements more accurately. At the end of each period the elements were corrected by the sums of the variations, and the corrected elements used for the variations for the next period. The total change in the elements having been thus determined, it was expected that the *Nautical Almanac* places would fairly represent observation.

The following Table of Errors, which are taken as referring to the nearest date for which a direct computation for the *Nautical Almanac* has been made, is formed from the observations at the Royal Observatory at Greenwich, and indicates that a correction of the elements is required.

		Cos $\delta . d\alpha$	$d\delta$
		"	"
1857	February 27	+ 1·05	+ 4·29
1858	June 10	+ 24·44	− 0·28
1859	September 21	− 10·44	− 6·40
1861	January 13	− 31·93	− 15·85
1862	April 26	+ 3·55	+ 7·05
1864	November 5	− 36·36	− 24·76
1866	January 23	− 55·14	+ 3·68
1867	May 24	− 16·03	+ 13·28
1868	September 3	− 41·85	− 12·64
1869	November 9	− 77·01	− 37·66
1871	March 22	− 55·99	+ 30·64
1872	July 2	− 43·77	+ 10·67
1873	September 25	− 73·57	− 39·29
1875	January 24	− 107·89	− 22·92
1876	April 30	− 61·46	+ 38·07

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EQUATIONS OF CONDITION.

Substituting their Logarithms for the Coefficients, and (dn) for 100 dn.

Right Ascension.

0 = +0·02167	+0·25343 de	−1·82727 (dn)	−9·42959 $d\pi$	−9·07791 $d\phi$	−8·74531 dv	+9·61091 di
0 = +1·38815	+0·15866	−1·67341	+8·79309	+0·45026	+8·71883	+8·26269
0 = −1·01880	+0·06763	−1·51698	+9·28063	−9·85385	−8·48159	+9·74006
0 = −1·50417	+0·13935	−1·51390	−8·88258	−0·40945	+8·80257	−8·47363
0 = +0·55082	+0·18784	−1·46086	−9·23279	+0·31537	+8·59062	+9·78662
0 = −1·56059	+0·06328	−1·04516	+9·06618	−0·28573	+8·62961	+9·74328
0 = −1·74145	+0·26956	−0·95626	−9·41007	−0·16827	−8·68744	+9·00651
0 = −1·20496	+0·16748	−8·81601	−8·10106	+0·46585	+8·89115	+9·34380
0 = −1·62167	+0·08108	+0·73824	+9·30775	−7·64933	−8·70851	+9·58870
0 = −1·88653	+0·15478	+1·11607	−7·75740	−0·45428	+8·85327	+9·41996
0 = −1·74809	+0·22490	+1·36820	−9·35468	+0·06218	−8·32243	+9·80386
0 = −1·64113	+0·14550	+1·41454	+9·08732	+0·37980	+8·15594	−8·79560
0 = −1·86670	+0·07492	+1·44078	+9·19673	−0·14660	−7·22272	+9·80866
0 = −2·03298	+0·20767	+1·64983	−9·19207	−0·37592	+8·24304	−8·99167
0 = −1·78857	+0·18405	+1·69760	−9·01030	+0·42486	+8·83916	+9·69184

Declination.

0 = +0·63246	−9·69624 de	+1·27768 (dn)	+8·87221 $d\pi$	+9·36738 $d\phi$	−8·97859 dv	+0·14955 di
0 = −9·44716	−9·60969	+1·12470	−8·23502	−9·90172	−9·45291	+8·81178
0 = −0·80618	+9·66285	−1·12072	+8·89691	−9·19385	+7·91116	−0·13094
0 = −1·20003	+9·70228	−1·07136	−8·52930	−9·94969	−9·40821	+8·93490

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$o = +0.84819$	-9.88424	$+1.16443$	$+8.96774$	-9.96629	$+9.12824$	$+0.08078$
$o = -1.39375$	$+9.75563$	-0.75036	$+8.80393$	-9.95905	-9.13716	-0.03589
$o = +0.56585$	-9.23843	$+0.02674$	$+8.29688$	$+9.41652$	-9.33933	$+0.02312$
$o = +1.12320$	-9.78392	$+9.10522$	$+7.98811$	-0.07942	$+9.41450$	$+9.72902$
$o = -1.10175$	$+9.53698$	$+0.11943$	$+8.76664$	$+9.10233$	$+8.92758$	-0.11871
$o = -1.57588$	$+9.76484$	$+0.71235$	$+7.68931$	-0.06718	-9.38773	-9.80866
$o = +1.48629$	-9.86102	-0.98924	$+9.01481$	-9.48451	$+8.29070$	$+0.15899$
$o = +1.02816$	-9.31789	-0.59749	-8.33385	-9.51965	$+9.43678$	-9.62017
$o = -1.59428$	$+9.75783$	$+1.11470$	$+8.91566$	-9.74813	-8.74013	-0.11989
$o = -1.36021$	$+9.48048$	$+0.93313$	-8.54506	-9.54760	-9.40890	$+9.76424$
$o = +1.58058$	-9.88245	-1.39200	$+8.77685$	-0.10034	$+9.30107$	$+9.99529$

Reducing by the method of least squares, the final equations are

$o = -3.00135$	$+1.56490\ de$	$-1.81720\ (dn)$	$-0.08089\ d\pi$	$+0.40170\ d\phi$	$-9.79403\ d\nu$	$+9.20734\ di$
$o = -4.15314$	-1.81720	$+4.27147$	$+0.93347$	$+1.15519$	$+0.22440$	$+0.26440$
$o = +1.37076$	-0.08089	$+0.93347$	$+9.65020$	-9.44460	-8.33700	-7.84111
$o = +2.73637$	$+0.40170$	$+1.15519$	-9.44460	$+1.85709$	-8.77328	$+9.52970$
$o = +1.45517$	-9.79403	$+0.22440$	-8.33700	-8.77328	$+9.77366$	$+9.27225$
$o = +1.69038$	$+9.20734$	$+0.26440$	-7.84111	$+9.52970$	$+9.27225$	$+1.25074$

whence

$de = +29''.26 + dn \times t$	$di = -2''.73$
$d\pi = +3''.13$	$d\phi = -8''.76$
$d\nu = -19''.73$	$dn = +0''.00871$

t being the number of days from 1867 May 24·0 G.M.T.

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RESIDUAL ERRORS.

		Equations.		Direct Calculation.	
		$\cos\delta . d\alpha$	$d\delta$	$\cos\delta . d\alpha$	$d\delta$
		"	"	"	"
1857	February 27	−4·8	+2·5	−4·8	+2·5
1858	June 10	−0·1	+0·6	−0·5	+0·6
1859	September 21	+1·1	+0·7	+1·1	+0·7
1861	January 13	+1·1	+1·1	+1·0	+1·3
1862	April 26	+2·4	−0·1	+2·3	−0·1
1864	November 5	+2·7	+0·8	+2·9	+0·8
1866	January 23	+4·2	−1·2	+4·3	−1·1
1867	May 24	−0·8	−0·4	−0·9	−0·3
1868	September 3	−1·2	−0·4	−1·2	−1·1
1869	November 9	−1·0	+0·6	−0·4	+0·7
1871	March 22	+1·3	−0·5	+1·4	−0·6
1872	July 2	−1·0	−0·3	−0·9	−0·1
1873	September 25	−3·7	−1·4	−4·0	−1·5
1875	January 24	−1·5	−0·1	−1·4	0·0
1876	April 30	+0·3	−1·1	+0·3	−1·4

The Centre of Gravity of the Apparent Disk of a Planet.

By Prof. Asaph Hall.

When a planet is observed from the Earth its disk will not be completely illuminated except at the time of opposition. At other times it will appear gibbous or crescent. If we assume the planet to be a sphere, which, according to the best observations is the case with *Mercury*, *Venus*, and *Mars*, the apparent disk will be composed of a semicircle and a semi-ellipse, which are separated by the line of cusps. If a right line be drawn through the centre of the line of cusps and at right angles to it, the centre of gravity of the apparent disk is in this line. In the plane triangle between the Sun, the Earth, and the planet, let φ be the angle at the planet, let a be the radius of the semicircular part of the disk, and denote by m the distance of the centre of gravity from the centre of the line of cusps. Then, when the planet is gibbous we shall have

$$m = \frac{8a}{3\pi} \sin^2 \tfrac{1}{2} \varphi,$$

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and when the planet is crescent,

$$m = \frac{8a}{3\pi} \cos^2 \tfrac{1}{2} \phi,$$

where π is the number 3·1416. The angle ϕ and the radius a can be computed from the data given in the Ephemerides, and the distance m is known, therefore, for any time.

In order to show the use of the quantity m, suppose the angle of position and distance of a satellite of a planet have been observed with a filar micrometer in the following manner. The wire is laid across the apparent disk of the planet in such a way that this disk is divided into two parts of equal area. The wire therefore always passes through the centre of gravity of the apparent disk, and it is necessary, before using the observations for computing orbits, to reduce these measures to the true centre of the planet. If we denote by p and s the observed angle of position and the distance of the satellite, and by θ the angle of position of the line of cusps, the errors of p and s arising from the unsymmetrical form of the disk will be

$$\Delta s = m . \sin (p - \theta),$$

$$\Delta p = \frac{m}{s} . \cos (p - \theta),$$

where Δp is expressed in parts of radius.

To complete the solution we have to find the value of θ. This is given from the spherical triangle between the pole of the Equator and the geocentric places of the Sun and the planet. In this triangle the angle at the planet is $90° - \theta$; and if we denote by d the angular distance between the Sun and the planet, by α, δ, α', δ' the geocentric right ascensions and declinations of the Sun and the planet, we shall have

$$\cos d = \sin \delta \sin \delta' + \cos \delta \cos \delta' \cos (\alpha' - \alpha),$$

$$\sin d \cos \theta = \cos \delta \sin (\alpha' - \alpha),$$

$$\sin d \sin \theta = \sin \delta \cos \delta' - \cos \delta \sin \delta' \cos (\alpha' - \alpha).$$

These equations give the angle θ without ambiguity.

In the case of a spheroidal planet, as *Jupiter* or *Saturn*, the apparent outline of the planet will be composed of ellipses, but the determination of these ellipses, though possible, is much more troublesome, and for these planets the correction for defective illumination is hardly sensible.

Washington, 1877, *November* 15.

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On a Portable Equatoreal Mounting for Small Telescopes.

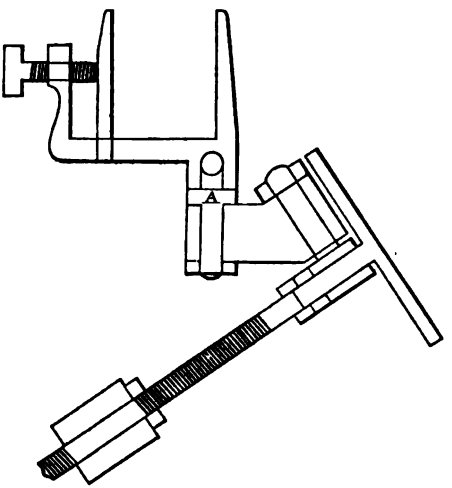
By Wentworth Erck, Esq.

This mounting is adapted for travelling astronomers or seaside visitors.

That now shown has been found quite strong enough to carry a $3\frac{1}{2}$-inch telescope of 50 inches focus, yet the mounting packs into a space of $9 \times 4\frac{1}{2} \times 1$ inch, and weighs less than 3 lbs.

It is evident that it can be attached to any window sash, either French or English; or to any post or rail, whether vertical or horizontal; and it will not injure a painted surface.

If it be attached to an English window sash, the sash must first be steadied by inserting a small wedge between the sash and the window frame. In this way it will give a far more extended and more comfortable view than the ordinary tripod stand.



When the mounting is to be attached to a vertical post or French window sash, then the vertical axis A is to be unscrewed out of the cubical block, and screwed into another face of the cube at right angles to the former face.

The mounting consists of a small vice, from which depends a vertical axis, on which turns the frame carrying the polar axis, so as to admit of adjustment in azimuth.

The present mounting being intended for use between London and Dublin only, it was not thought necessary to make any provision for altering the inclination of the polar axis. This can be done, if desired, by introducing another joint between the polar and vertical axes. All the screw-heads and nuts are of the same size, so that they can be tightened by the same wrench.

1878, *January* 9.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. FEBRUARY 8, 1878. No. 4.

WILLIAM HUGGINS, Esq., F.R.S., President, in the Chair.

Henry Baumer, Esq., 27 Harley Street, W.;
Daniel Thomas Evans, Esq., 10 Kent Gardens, Castle Hill Park, Ealing, and 5 Elm Court, Temple;
John E. Griffith, Esq., Bangor;
Henry Burdett Hederstedt, Esq., M.I.C.E., 72 Lancaster Gate, W., and Lucknow, India;
Frederic G. Landon, Esq., M.A., 8 The Circus, Greenwich, S.E.;
John Ambrose Ridgway, Esq., Foundation School, Beverley;
Benjamin Templar, Esq., Birkdale, Southport; and
Richard Wilding, Esq., Associate Inst. of Actuaries, Cobridge, near Stoke-upon-Trent;

were balloted for and duly elected Fellows of the Society.

REPORT OF THE COUNCIL TO THE FIFTY-EIGHTH ANNUAL GENERAL MEETING OF THE SOCIETY.

Progress and present state of the Society:—

	Compounders.	Annual Subscribers.	Non-resident.	Mathematical Society.	Patroness.	Total Fellows.	Associates.	Grand Total.
December 31, 1876	212	353	5	7	1	578	42	620
Since elected ...	+9	+20	...	...	...	...	...	...
Deceased	−6	− 6	−1	−1	...	...	−5	...
Removals	+3	− 3	...	...	...	...	...	...
Resigned	...	−11	...	...	...	...	...	...
Expelled	...	− 2	...	...	...	...	...	...
December 31, 1877	218	351	4	6	1	580	37	617

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Mr. Whitbread's Account as Treasurer of the Royal

RECEIPTS.

	£	s.	d.	£	s.	d.
Balance at Bankers', Dec. 31, 1876, as per Bankers' book ... £253 1 10						
Less amount of outstanding cheque 15 0 0						
	238	1	10			
„ in hand of Secretary of Library Committee:						
On account of Library Expenses ...	13	8	6			
„ Turnor Fund	22	3	9			
				273	14	1
Dividend on £4,200 Consols	62	4	3			
„ £5,200 New 3 per cent. Stock ...	77	0	6			
„ £6,400 Consols	94	16	0			
„ £5,700 New 3 per cent. Stock ...	84	8	8			
				318	9	5
Received on account of Subscriptions:						
Arrears of Contributions	108	3	0			
253 Contributions for 1877	531	6	0			
On account of ditto	2	0	0			
3 Contributions for 1878	6	6	0			
27 Admission Fees	56	14	0			
19 First Contributions	31	10	0			
				735	19	0
10 Composition Fees	...		...	210	0	0
Sales of Publications:						
At Society's Rooms, during 1877	43	10	9			
At Williams & Norgate's, during 1876 ...	94	4	9			
				137	15	6
Bequest of the late C. Lambert, Esq.	...		...	500	0	0
				£2,175	18	0

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Astronomical Society from Dec. 31, 1876, to Dec. 31, 1877.

EXPENDITURE.

	£ s. d.	£ s. d.
Salaries:—		
Editor of *Monthly Notices*	60 0 0	
Assistant Secretary	150 0 0	
		210 0 0
Income Tax and House Duty		6 11 3
Fire Insurance		7 16 6
Printing:—Spottiswoode & Co.		280 9 6
Engraving:—W. H. Wesley		11 4 0
Library expenses: Binding		93 0 0
Turnor Fund: Books purchased during year ...		20 16 10
Miscellaneous:—		
House expenses	31 12 6	
Wages	23 8 0	
Stamps and postage	53 12 5	
Carriage of books and parcels	5 13 9	
Stationery and office expenses	9 14 1	
Expenses of meetings	17 15 6	
Coals and gas	48 11 9	
Fittings and repairs	6 14 2	
Mrs. Jackson-Gwilt's annuity	8 19 0	
Sundries	6 0 4	
		212 1 6
Paid on account of Mr. Gill's expedition to Ascension		400 15 6
Investments:—		
Purchase of £200 Consols, at 96½, including Commission	193 5 0	
Purchase of £500 New 3 per cent. Stock at 95, including Commission	475 12 6	
		668 17 6
Balance at Bankers', Dec. 31, 1877, as per Bankers' book ... £240 19 9		
Less amount of outstanding cheque ... 15 0 0		
	225 19 9	
„ in hand of Secretary of Library Committee:		
On account of Library Expenses ...	20 8 6	
„ Turnor Fund	1 6 11	
„ in hand on Petty Cash account	13 11 7	
		261 6 9
„ due to Assistant Secretary on Petty Cash account Dec. 31, 1876		2 18 8
		£2,175 18 0

Examined and found correct.

J. Kennedy Esdaile.
A. Ainslie Common.
Wentworth Erck.

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Assets and present property of the Society, January 1, 1878:—

	£ s. d.	£ s. d.
Balance at Bankers', Dec. 31, 1877	240 19 9	
Less amount of outstanding cheque	15 0 0	
		225 19 9
Due on account of Subscriptions:—		
1 Contribution of 6 years' standing	12 12 0	
1 " 5 "	10 10 0	
6 " 4 "	50 8 0	
12 " 3 "	75 12 0	
36 " 2 "	151 4 0	
45 " 1 "	94 10 0	
Various amounts	10 14 0	
2 Admission Fees and First Contributions ...	6 6 0	
	411 16 0	
Less 3 Contributions paid in advance	6 6 0	
		405 10 0
Due for Publications		85 6 6
Due from Guarantors of Fund for the Expedition to Ascension		150 15 6
Balance in hand of Secretary of Library Committee on account of Library Expenses	20 8 6	
Do. do. on account of Turnor Fund ...	1 6 11	
In hand on Petty Cash account	13 11 7	
		35 7 0

£5,700 New 3 per cent. Stock, including Mrs. Jackson-Gwilt's gift (£300).
£6,400 Consols, including the Lee Fund (£300), the Turnor Fund (£450), and the Horrox Memorial Fund (£100).
Astronomical and other MSS., Books, Prints, &c.
Various Astronomical Instruments, Models, &c.
Two Gold Medals.
Unsold Publications of the Society &c.

Examined and found correct.

J. Kennedy Esdaile.
A. Ainslie Common.
Wentworth Erck.

REPORT OF THE AUDITORS.

We, the undersigned Auditors, appointed at the January 1878 meeting of the Royal Astronomical Society, beg to lay before this General Meeting the following Report:—

1. We have examined the Treasurer's account, and an account of the assets and other property of the Society, and have found and certified the same to be correct.

2. The receipts and expenditure for the past year are as stated in the Treasurer's account.

3. The balance in hand, including cash at bankers', amounts to 261*l.* 6*s.* 9*d.*

4. The funded property of the Society is in a satisfactory state, and has been increased during the past year by the sums of 500*l.* New 3 per Cents. and 200*l.* Consols. This latter amount

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may be taken to include a sum of 121*l.* 18*s.* 8*d.*, being the amount of accumulated interest of the Turnor Fund now available for the specified purpose of the trust. The books, instruments, and other properties have been examined as far as circumstances would allow, and so far found in a satisfactory condition.

5. We have laid on the table a list of the names of those Fellows who are in arrear for sums due at the last Annual General Meeting, together with the amounts of the same.

J. Kennedy Esdaile,
A. Ainslie Common,
Wentworth Erck.

Stock in hand of volumes of the *Monthly Notices*:—

Vol.	At Society's Rooms.	At Williams & Norgate's.	Vol.	At Society's Rooms.	At Williams & Norgate's.
I.	76	2	XXI.	25	...
II.	97	2	XXII.	41	...
III.	...	...	XXIII.	25	...
IV.	...	...	XXIV.	27	...
V.	...	...	XXV.	8	...
VI.	54	1	XXVI.	15	...
VII.	3	...	XXVII.	2	...
VIII.	146	3	XXVIII.	82	2
IX.	28	3	XXIX.	65	1
X.	181	2	XXX.	76	5
XI.	189	2	XXXI.	112	3
XII.	15	2	XXXII.	145	4
XIII.	167	3	XXXIII.	128	4
XIV.	112	3	XXXIV.	103	7
XV.	132	2	XXXV.	29	5
XVI.	114	3	XXXVI.	59	1
XVII.	140	1	XXXVII.	73	3
XVIII.	172	...			
XIX.	71	...	Index to *Monthly Notices*	610	...
XX.	42	...			

In addition to the above volumes of the *Monthly Notices*, the Society has a considerable stock of separate numbers of nearly all the volumes. With the exception, however, of Vols. XXXV., XXXVI., and XXXVII., no complete volumes can be formed from the separate numbers in stock. In order to facilitate the

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completion of sets, the Council has, during the past session, resolved that separate numbers are to be sold at a shilling each, and in the case of volumes which can be supplied complete by the Society, the price of any number of parts of a single volume sold at any one time is not to exceed one shilling less than the price of the complete volume. With regard to the numbers of any volume of which the Society cannot supply the complete volume, all the numbers which can be supplied are to be sold for ten shillings to the public—a discretionary power being always left to the Assistant-Secretary to modify these rules in exceptional cases.

Stock in hand of volumes of the *Memoirs* :—

Vol.	At Society's Rooms.	At Williams & Norgate's.	Vol.	At Society's Rooms.	At Williams & Norgate's.
I. Part 1	3	...	XXII.	164	2
I. Part 2	48	...	XXIII.	160	1
II. Part 1	56	...	XXIV.	166	2
II. Part 2	23	...	XXV.	179	2
III. Part 1	71	...	XXVI.	182	2
III. Part 2	93	...	XXVII.	435	2
IV. Part 1	88	3	XXVIII.	397	1
IV. Part 2	98	3	XXIX.	424	2
V.	113	4	XXX.	173	1
VI.	132	4	XXXI.	158	1
VII.	156	3	XXXII.	180	1
VIII.	134	4	XXXIII.	179	3
IX.	143	3	XXXIV.	178	9
X.	154	1	XXXV.	129	3
XI.	166	1	XXXVI. (with M.N.)	209	15
XII.	170	...	XXXVI. (without)	17	...
XIII.	180	...	XXXVII. Part 1	376	9
XIV.	380	3	XXXVII. Part 2	324	6
XV.	150	1	XXXVIII.	322	...
XVI.	180	...	XXXIX. Part 1	305	2
XVII.	156	4	XXXIX. Part 2	322	1
XVIII.	160	1	XL.	358	4
XIX.	165	1	XLII.	358	...
XX.	163	2	XLIII.	487	11
XXI. Part 1	314	...	Index to *Memoirs*	676	4
XXI. Part 2	99	...			
XXI. 1 & 2 (together)	69	2			

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Instruments belonging to the Society.

The following is a complete list of the instruments in the Society's apartments. A valuable Double Image Micrometer, which is given as No. 85 in the list, was presented to the Society in the course of the past year by Mr. Simms.

A circular has been sent to all persons having instruments belonging to the Society in their possession, in accordance with the resolution of the Council, mentioned in the last Annual Report.

The list of instruments is as follows:—

No. 1. The *Harrison* clock,
„ 2. The *Owen* portable circles, by Jones,
„ 3. The *Beaufoy* circle,
„ 4. The *Beaufoy* transit instrument,
„ 5. The *Herschelian* 7-foot telescope,
„ 6. The *Greig* universal instrument, by Reichenbach and Ertel. The transit telescope, by Ultzschneider and Fraunhofer, of Munich,
„ 7. The *Smeaton* equatoreal,
„ 8. The *Cavendish* apparatus,
„ 9. The 7-foot Gregorian Telescope (late Mr. Shearman's),
„ 10. The Variation transit instrument (late Mr. Shearman's),
„ 11. The Universal quadrant, by Abraham Sharp,
„ 12. The *Fuller* theodolite,
„ 13. The Standard scale, by Troughton and Simms,
„ 14. The *Beaufoy* clock, No. 1,
„ 15. The *Beaufoy* clock, No. 2,
„ 16. The *Wollaston* telescope,
„ 17. The *Lee* circle,
„ 18. The *Sharpe* reflecting circle,
„ 19. The *Brisbane* circle,
„ 20. The *Baker* universal equatoreal,
„ 21. The *Reade* transit,
„ 22. The *Matthew* equatoreal, by Cooke,
„ 23. The *Matthew* transit instrument,
„ 24. The *South* transit instrument,
„ 25. A Quadrant, by Bird (formerly belonging to Captain Cook),
„ 26. A globe showing the Precession of the Equinoxes.

The *Sheepshanks* collection:—

„ 27. (1.) 30-inch transit instrument, by Simms, with level and two iron stands.
„ 28. (2.) 6-inch transit theodolite, with circles divided on silver; reading microscopes, both for altitude and azimuth; cross and siding levels; magnetic needle; plumbline; portable clamping foot and tripod stand.

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No. 29. (3.) $4\frac{6}{10}$-inch achromatic telescope, about 5 feet 6 inches focal length; finder; rack motion; double-image micrometer; two other micrometers; one terrestrial and ten astronomical eyepieces, applied by means of two adapters, with equatoreal stand, clock movement.

„ 30. (4.) $3\frac{1}{4}$-inch achromatic telescope, with equatoreal stand; double-image micrometer; one terrestrial and three astronomical eyepieces.

„ 31. (5.) $2\frac{3}{4}$-inch achromatic telescope, with stand; one terrestrial and three astronomical eyepieces.

„ 32. (6.) $2\frac{3}{4}$-inch achromatic telescope, about 30 inches focal length; one terrestrial and four astronomical eyepieces. This instrument was lent to the late Rev. J. Cape, and is believed to be lost.

„ 33. (7.) 2-foot navy telescope.

„ 34. (8.) A transit instrument of 45 inches focal length; with iron stand, and also Ys for fixing to stone piers; two axis levels.

„ 35. (9.) Repeating theodolite, by Ertel, with folding tripod stand.

„ 36. (10.) 8-inch pillar sextant, by Troughton, divided on platinum, with counterpoise stand and artificial horizon.

„ 37. (11.) Portable zenith telescope and stand, $2\frac{3}{4}$-inch aperture and 26 inches focal length; 10-inch horizontal circle and 8-inch vertical circle, read to 10″ by two verniers to each circle.

„ 38. (12.) 18-inch Borda repeating circle, by Troughton, $2\frac{1}{8}$-inch aperture and 24 inches focal length; the circles divided on silver, the horizontal circle being read by four verniers, and the vertical circle by three verniers, each to 10″.

„ 39. (13.) 8-inch vertical repeating circle, with diagonal telescope, by Troughton and Simms; circle divided on silver, reading to 10″; a 5-inch circle at eye-end reading to single minutes; horizontal circle 9 inches diameter in brass, reading to single minutes.

„ 40. (14.) A set of surveying instruments, consisting of a 12-inch theodolite for horizontal angles only, reading to 10″; two sets of adjusting plates; tripod stand with enclosed telescope; a deal box with heavy stand for theodolite; a box containing the Y piece of level; two large and three small ground-glass bubbles divided; a box containing level collimator, object-glass $1\frac{5}{8}$-inch diameter and 16 inches focal length; micrometer eyepiece, comb, and wires.

„ 41. (15.) Level collimator with object-glass $1\frac{7}{8}$-inch diameter and 16 inches focal length; stand, rider-level, and fittings.

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No. 42. (16.) 10-inch reflecting circle, by Troughton, reading by three verniers to 20″; counterpoise stand; artificial horizon with mercury; two tripod stands.
„ 43. (17.) Hassler's reflecting circle, by Troughton, with counterpoise stand.
„ 44. (18.) 6-inch reflecting and repeating circle, by Troughton and Simms, contained in three boxes, two of which form stands. The circle is divided on silver, and is read to single minutes; two inside arcs divided to single degrees, 150 degrees on each side; artificial horizon and mercury.
„ 45. (19.) 5-inch reflecting and repeating circle, by Lenoir, of Paris.
„ 46. (20.) Reflecting circle, by Jecker, of Paris, 11 inches in diameter, with one vernier reading to 15″.
„ 47. (21.) Box sextant; reflecting plane and level.
„ 48. (22.) Prismatic compass, by Troughton and Simms.
„ 49. (23.) Mountain barometer.
„ 50. (24.) Prismatic compass, by Thomas Jones, mounted with a cylindrical lens.
„ 51. (25.) Ordinary 4½-inch compass with needle.
„ 52. (26.) Dipping needle, by Robinson.
„ 53. (27.) Compass needle, mounted for variation.
„ 54. (28.) Magnetic intensity needle, by Meyerstein, of Göttingen; a strongly fitted brass box with heavy magnet; filar suspension.
„ 55. (29.) Box of magnetic apparatus.
„ 56. (30.) Hassler's reflecting circle, by Troughton; a 10½-inch reflecting and repeating circle, with stand and counterpoise, divided on platinum with two movable and two fixed indices; four verniers reading to 10″.
„ 57. (31.) Box sextant and glass plane artificial horizon, by Troughton and Simms.
„ 58. (32.) Plane 2⅜-inch speculum, artificial horizon, and stand.
„ 59. (33.) 2½-inch circular level horizon, by Dollond.
„ 60. (34.) Artificial horizon, roof, and trough; the trough 8¼ by 4¼ inches.
„ 61. (35.) Set of drawing instruments, consisting of 6-inch circular protractor and common protractor, T-square: one beam compass.
„ 62. (36.) A pentagraph.
„ 63. (37.) A noddy.
„ 64. (38.) A small Galilean telescope, with object-glass of rock crystal.
„ 65. (39.) Five levels.
„ 66. (40.) 18-inch celestial globe.
„ 67. (41.) Varley stand for telescope.
„ 68. (42.) Thermometer.

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No. 69. (43.) Telescope, with the object-glass of rock crystal.
„ 70. Portable equatoreal stand.
„ 71. Portable altazimuth tripod.
„ 72. Four polarimeters.
„ 74. Registering spectroscope, with one large prism.
„ 76. Two five-prism direct-vision spectroscopes.
„ 78. 9¼-inch silvered-glass reflector and stand, by Browning.
„ 79. Spectroscope.
„ 80. A small box, containing three square-headed Nicol's prisms; two Babinet's compensators; two double-image prisms; three Savarts; one positive eyepiece, with Nicol's prism; one dark wedge.
„ 81. A back-staff, or Davis' quadrant.
„ 82. A nocturnal, or star dial.
„ 83. An early non-achromatic telescope, of about 3 feet focal length, in an oak tube made in one piece. The words "Samuel Scatliffe, Londini, Fecit," are engraved on the draw-tube of the eyepiece.
„ 84. A Hollis observing chair.
„ 85. A double image micrometer, by Troughton and Simms.

The following instruments, which were not included in the list published in last year's Annual Report, are also in the possession of the Society:—

No. 86. A 4½-inch Gregorian Reflecting Telescope, by Short, of London, with an altazimuth stand with 6-inch divided altitude and azimuth circles, extra small mirror, and two eyepieces.
„ 87. A 3¼-inch Gregorian Reflecting Telescope with wooden tripod stand.
„ 88. A pendulum with 5-foot brass suspension rod, working on knife edges, by Thomas Jones. This was one of the instruments used by Sir E. Sabine in his pendulum experiments.
„ 89. A Rhabdological Abacus. A contrivance invented by Mr. H. Goodwyn. It consists of a box filled with compartments, in which are square rods covered with numbers, which can be arranged so as to facilitate the labour of multiplying high numbers.
„ 90. A bronze celestial globe, not quite six inches in diameter, covered with Arabic characters. The stars are represented by small inlaid pieces of whiter metal. The globe is surrounded by a metal divided circle, also engraved with Arabic characters.
„ 91. An astronomical time watch-case, by Professor Chevallier, being a box with a lid with a circular aperture, through which the hands of a watch may be seen. Upon the lid is a plate engraved with a double circle of hours, which may be set so as to in-

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dicate sidereal time when the watch is going to ordinary mean time.

No. 92. A 2-foot protractor, with two movable arms, and vernier.

„ 93. A beam compass, in box.

„ 94. A 2-foot navigation scale.

„ 95. Stand for testing measures of length. It consists of two T-shaped gun-metal bars 5½ feet long, to which are fitted two micrometers mounted with adjusting screws for level and position.

The following instruments are lent, during the pleasure of the Council, to the undermentioned persons:—

No. 2. The *Owen* portable circle, No. 1, to Mr. Lecky.

„ 4. The *Beaufoy* transit instrument, to the Observatory, Kingston, Canada.

„ 12. The *Fuller* theodolite, to the Director of the Sydney Observatory.

„ 22. The *Matthew* equatoreal, to Mr. Brett.

„ 23. The *Matthew* transit, to Mr. Gill, for the Expedition to Ascension.

„ 74. The registering spectroscope, with prism, to Mr. Lecky.

„ 81. *Davis'* quadrant, to Mr. Lecky.

From the *Sheepshanks* collection:—

„ 31. No. 5, to Mr. Birt.

„ 34. No. 8, to the Rev. Professor Pritchard.

„ 35. No. 9, to the Sydney Observatory.

„ 43. No. 17, to Mr. Gill, for the Expedition to Ascension.

„ 67. No. 41, to the Rev Professor Pritchard.

„ 69. No. 43, to Dr. Huggins.

Missing Instruments.

Instruments Nos. 64, 66, and 71, which were mentioned in the last *Annual Report* as missing, have been found, but the Council have to regret that no information with regard to the remaining five instruments which appear in the last two *Annual Reports* as missing has been obtained. During the lifetime of the late Assistant Secretary no book was kept for the purpose of recording the loan and return of instruments belonging to the Society, but some letters applying for the loan of instruments, and memoranda made by the late Mr. Williams upon separate pieces of paper, giving information with respect to the instruments lent by the Society, have been preserved. These have been carefully examined, but no further information has been derived with regard to the missing instruments. The Council have therefore directed that the following instruments shall be removed from the list:—

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No. 11. Universal Quadrant, by Abraham Sharp. (First mentioned in the *Report* of the Council published in 1847. There is no record of the instrument having been lent, and no information with regard to it can be obtained.)

No. 22. The *Reade* Transit. (First mentioned in the Council's *Report* for 1870. No information can be obtained with regard to it, and there is no record of its having been lent.)

No. 32. *Sheepshanks* instrument No. 6. (Lent to the Rev. Jonathan Cape in 1858. There is no entry of its return to the Society. Mr. Cape died in 1872, and his instruments were sold by auction shortly afterwards, but all efforts which have been made to trace the instrument have been unsuccessful. *See* p. 127 of Vol. XXXVI. of the *Monthly Notices.*)

No. 68. A Thermometer. (This is one of the instruments presented to the Society by Miss Sheepshanks. No information can be obtained with regard to it.)

The Gold Medal.

The Council have awarded the Society's Gold Medal to Baron Dembowski, of Gallarate, near Milan, for his Researches on Double Stars. The President will lay before the Meeting the grounds upon which the Council have decided upon their award.

Mr. Lambert's Bequest.

During the past year the treasurer has received the sum of 500*l.* from Mr. C. J. Lambert, a son of the late Mr. C. Lambert, F.R.A.S., whose name appears in the list of deceased Fellows mentioned in last year's Annual Report. Mr. C. Lambert left by his will a sum of 25,000*l.* at the disposal of his son to be distributed amongst persons who had been in his employ and in gifts to scientific societies. Out of this sum Mr. C. J. Lambert appropriated to the Astronomical Society the sum of 500*l.*, which was paid free of legacy duty. The Council has thus the pleasure of reporting that the funded property of the Society has, within the course of the last two years, been increased by legacies amounting to 2,580*l.*, viz. the Janson legacy, 200*l.*; the Carrington legacy, 2,000*l.* Consols, valued at 1,880*l.*; and the Lambert bequest, 500*l.*

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Mr. Gill's Expedition for observing the Opposition of Mars.

During the past year Mr. David Gill applied to the Council to aid him in carrying out an expedition which he proposed to make to the Island of Ascension for observing the opposition of *Mars* that was about to take place. Lord Lindsay had placed at his disposal the Heliometer which he had made use of in observing the opposition of *Juno* in the Mauritius, and Mr. Gill offered, in addition to giving his own services, to defray any expenses which might be incurred beyond 500*l.*

The value of this opposition for determining the Sun's distance may be gathered from the following numbers, showing the distance of *Mars* from the Earth expressed in the unit of the Earth's distance from the Sun at the various oppositions of the planet till the year 1892. At the opposition of 1877 the distance of *Mars* was ·37; in 1879 it will be ·48; in 1881–2, ·60; in 1884, ·67; in 1886, ·76; after which the distance continually diminishes till the opposition of 1892, when it will be reduced to ·38.

The Council, having regard to the favourable conditions of this opposition and to the trifling cost of such an expedition as compared with the expenditure necessary for the observation of a transit of *Venus*, as well as to the circumstances connected with Mr. Gill's offer, resolved to grant a sum of 250*l.* out of the funds of the Society towards the expenses of the expedition, and they are happy to be able to state that the remaining 250*l.* has since been granted out of the Government fund at the disposal of the Royal Society. Fellows of the Society will be aware, from the reports which have from time to time appeared in the pages of the *Monthly Notices*, that the expedition has been carried out with much energy by Mr. Gill and has been brought to a successful issue.

The Library.

During the past year the binding of the Society's books has been systematically proceeded with. Five hundred and one volumes have been bound in a substantial manner. Most of the publications of Observatories, and the series of Transactions of Learned Societies, are now bound; but there still remains a large number of tracts and volumes which require binding.

One hundred and twenty-two volumes (including tracts) have been purchased out of the Turnor Fund during the past year. Some of the *lacunæ* in serial publications have been filled up, but many still remain; and the Council take this opportunity of drawing the attention of Fellows possessing odd numbers of the *Astronomische Nachrichten* and other astronomical publications,

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to the list of missing numbers and volumes of serials, which may be seen on application to the Assistant-Secretary.

The Library Committee is anxious to complete, as far as possible, the Society's collections of Star Catalogues, Atlases, and other works of astronomical reference. It is also thought desirable that the library should contain the chief modern English and Foreign books upon popular astronomy, and it is hoped that in the future the library will be rendered much more complete in this respect by means of donations as well as purchases. Such books are very frequently borrowed from the library by Fellows, and in the case of some of the popular treatises it would be an advantage if the Society possessed duplicate copies, one for reference in the library and one for loan.

The number of books taken out of the library during the past year is greatly in excess of the number taken out in any previous year, and the Council congratulate themselves that the library is also much more freely used than formerly for purposes of reference.

Only one application for the loan of MSS. belonging to the Society has been received during the past year, but the thirty-nine volumes of Heinrich Schwabe's Sun Spot MSS. are still at the Kew Observatory.

The regulation of the Council that "It shall be the duty of the Assistant-Secretary, before the November meeting, to forward to all Fellows of the Society who, according to the entries in the book kept for that purpose, have any books belonging to the Society in their possession, a letter requesting that all such books be returned to the Society on or before the day of the November meeting," has been carried out, and in every instance the books have either been returned or answers have been received acknowledging their possession and requesting a further loan of them.

Publications of the Society.

Of vol. xli. of the *Memoirs*, which relates to observations made during total solar eclipses, the first four hundred and eighty pages are printed. The remainder of the volume, which will amount to rather more than two hundred pages, is already partly in type, and the Council have reason to hope that the complete volume will be in the hands of the Fellows in the course of two or three months. The part already printed contains collected accounts of observations made previously to totality; observations made about the time of the commencement and towards the end of totality; observations relating to the brightness of the corona; polariscopic, spectroscopic, and some other classes of observations made during totality.

The part which is not yet printed will contain a discussion of the principal drawings and photographs of the corona made up

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to the year 1871; a catalogue of the eclipse observations which have been referred to in compiling the volume; a general Index; and seventeen quarto plates of photographs of the corona, and of the structure visible in the corona photographs of 1871.

Volume xliii. of the *Memoirs* was published in November. It contains the six following Papers:—

1. *The Chronology of Star Catalogues.* By E. B. Knobel, Esq.

This paper contains a list of 530 star catalogues arranged in order of epoch, and gives very complete information with regard to the number of stars in each catalogue, the date of publication, the magnitudes and special peculiarities of the stars described, particulars as to their positions and the coordinates given, the epoch which is the basis of the arrangement adopted, and general notes, with references to the works in which the catalogues are to be found.

All the star catalogues in the list, with the exception of a few ancient and Oriental catalogues, have been personally examined by Mr. Knobel, and his notes upon the earlier Arabian and Persian astronomers contain much curious information.

The table of catalogues is followed by a list of twenty-nine catalogues of proper motions of stars, and by Aboul Hhassan's "Table of the longitudes and latitudes of 240 stars for the commencement of the Hegira," with critical notes on the stars, and a list of Arabic errata in the translation of this catalogue and that of Ulugh Beigh.

II. *Micrometrical Measures of Double Stars.* By George Knott, Esq.

III. *Second Catalogue of Micrometrical Measures of Double Stars made at the Temple Observatory.* By J. M. Wilson, M.A., and G. M. Seabroke, Esq.

IV. *Theory of the Horizontal Photoheliograph, including its application to the determination of the Solar Parallax by means of Transits of Venus.* By Prof. William Harkness, U. S. Navy.

V. *The Sidereal System.* By Maxwell Hall, M.A., of Pembroke College, Cambridge.

VI. *Mémoire sur la Période commune à la Fréquence des Taches Solaires et à la Variation de la Déclinaison Magnétique.* Par M. le Docteur Rudolf Wolf.

This paper contains the general results of Dr. Wolf's researches in connection with the periodicity in the development of Sun-spots, and is accompanied by a diagram showing the form of the Sun-spot curve as determined from records which Dr. Wolf has examined, dating from 1745 to the present time. A table is also given, showing the dates of maxima and minima of Sun-spot development from 1610 to the present time. From this table Dr. Wolf shows that the length of the mean Sun-spot period is 11·111 years, and that the average divergence of maxima and

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minima from the mean period is 2·030 years. The paper contains a list of the MSS. and printed records which Dr. Wolf has consulted in compiling the above table and curve, and shortly explains the methods he has adopted in reducing the observations of so many observers to the same tabular form.

Most of the information given in the paper has already been published in German in the pages of the *Astronomische Mittheilungen*, but the Council hope that the astronomical public will welcome Dr. Wolf's important results in a more concise and accessible form than they have hitherto appeared.

In the numbers of the *Monthly Notices* published during the past year many valuable papers have appeared, amongst which will be noticed several communications from foreign astronomers.

Retirement of Treasurer.

The Council desire to express their regret that Mr. S. C. Whitbread should have been compelled by ill health to resign the office of Treasurer, which he has held for the last twenty years so much to the advantage of the Society. The Council have expressed to Mr. Whitbread their sense of the value of the services that he has rendered to the Society, and their sympathy with him in his enforced retirement from the office of Treasurer.

Obituary.

The Council regret that they have to record the loss by death of the following Fellows and Associates during the past year:—

Fellows:—Rev. H. A. S. Atwood.
Sir Edward Belcher.
J. W. Bosanquet.
J. S. Bowerbank.
O. G. Downes.
J. L. Kenworthy.
Captain H. O'Reilly.
Rev. W. L. Onslow.
W. J. Rideout.
Rev. F. W. Russell.
W. H. Fox Talbot.

Associates:—Dr. C. Bremiker.
Dr. E. Heis.
U. J. J. Le Verrier.
Prof. K. von Littrow.
Prof. G. Santini.

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The Rev. HENRY ADAMS SERGISON ATWOOD was born on January 13, 1800, in the parish of St. Margaret's, Westminster, where his father, who died young, was curate. His father was a grandson of the Rev. George Atwood, Archdeacon of Taunton, and nephew of George Atwood, Fellow and Tutor of Trinity College, Cambridge, who invented the well-known contrivance called Atwood's machine, which is still used for lecture-room experiments to demonstrate the laws of uniformly accelerated motion. Henry Adams Sergison Atwood was entered at Queen's College, Oxford. He took his M.A. degree in 1824, and shortly afterwards, having obtained orders, he became curate first of Banbury and then of Kenilworth. In 1839 he was collated to the rectory of Ashelworth, in the county of Gloucester, where, in addition to the duties of his parish, he devoted himself most successfully to tuition. Several of his pupils have since taken distinguished positions in the political world. In 1827 he was elected a Fellow of the Astronomical Society, and, consequently, at the time of his death he was one of the oldest Fellows of the Society. He was always devotedly attached to the study of astronomy, though his many duties prevented him from indulging his tastes, as he no doubt would have done if he had been less occupied with his parish and his pupils. In 1860 he accompanied the expedition which was sent out in H.M.S. *Himalaya* to observe the total eclipse of the Sun in Spain. At the time of totality Mr. Atwood was stationed, together with Mr. Francis Galton and Mr. Gray, on a tower outside the little town of La Guardia, near Logroño, from which they had an excellent view of the surrounding country. It had been arranged that Mr. Atwood should devote himself to meteorological observations, but he also found time to observe the changes of tint upon the clouds and sky as the shadow advanced. During totality he observed the positions of several of the red prominences through a small telescope, and scrutinised the structure of the corona. Immediately after totality was over he was fortunate in observing the rays from the cusps of the thin solar crescent which have only been seen and described by a few other observers. An account of his observation is given in vol. xli. of the *Memoirs*. Mr. Atwood died on June 22, 1877.

ADMIRAL SIR EDWARD BELCHER, K.C.B., was born in the year 1800. His great grandfather, Jonathan Belcher, was Governor of Massachusetts before the War of Independence, and his grandfather, also named Jonathan Belcher, was chief justice of Nova Scotia. Edward Belcher first brought himself into public notice when a lieutenant on board the surveying ship *Blossom*. In a voyage which lasted nearly four years he visited and assisted materially in fixing the Astronomical positions of various islands, and expunged from the charts many supposed dangers which the uncertainties of longitude had repeated in several positions, and which had till then proved a source of

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anxiety and perplexity to the ordinary navigator. Having left the coast of America, the *Blossom* visited Easter, Deucie, and the Pitcairn Islands, and proceeded thence to the Gambier Islands, which were surveyed in considerable detail. Thence they sailed to the Coral Isles of Polynesia, where several new islands were discovered and the positions of others correctly determined.

In April 1826 the *Blossom* received orders to sail for the Behring Straits, *via* the Sandwich Islands and Kamtchaka; a portion of her captain's instructions being that she should pass the summers of 1826–27 in this region, in order to co-operate with Parry and Franklin, then exploring in the Arctic Seas, in case either of them should succeed in accomplishing the North-west passage. In July 1826 the ship reached Kotzebue Sound, when, with the assistance of her decked boat, a survey of coast to the north was commenced, the ship herself reaching a point named Cape Franklin in 71° 7′ N. latitude and her barge a position considerably further advanced. Foiled in their expectation of meeting Franklin's land party, although the boat had reached within 120 miles of his furthest western position, the *Blossom* quitted Behring Straits, and returned to resume her survey duties in the Pacific until the time should again come round for a second attempt. During the next few months they visited San Francisco, the Sandwich Islands, and the Loo Choo Islands. Passing thence northward, Kotzebue Sound was again reached on the 5th August 1827. Here the decked boat was prepared for a second voyage of exploration to the north, and under Lieut. Belcher's command the coasts of Chamisso Island were examined to a position beyond Icy Cape, a distance of between three and four hundred miles. On his return to Kotzebue Sound, after experiencing many risks, his little vessel was driven on shore in a gale of wind and totally wrecked on Chamisso Island, three of her crew being drowned.

The *Blossom* now finally quitted the Behring Straits, all hope of obtaining the principal object of her mission, viz. the meeting with Franklin's expedition, being at an end. Having visited California, she returned to England in October 1828.

Lieutenant Belcher was promoted to the rank of commander in 1829, and in 1830 he was appointed to the command of the surveying ship *Etna*, employed in surveying the West Coast of Africa and parts of the Mediterranean. Having surveyed the dangerous shoals which extend some seventy miles off the coast in the neighbourhood of Rio Grande, south of the Gambia, he was ordered to pass the winter of 1832 within the bar of the Duero River, for the protection of British interests during the struggles between the parties of Dons Pedro and Miguel. While engaged in this work he observed the transit of *Mercury* of May 5, 1832. An account of his observations is given in vol. ii. of the *Monthly Notices*.

Having returned to England in 1833, he was employed for some time on the survey of the coasts of the United Kingdom,

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and in 1837 he was appointed to the command of a surveying expedition in the Pacific, composed of H.M.S. *Sulphur* and *Starling*. After five years' work, during which he succeeded in establishing several important geographical facts, he returned to England in July 1842, and was nominated a Companion of the Bath, and shortly after received the honour of knighthood.

In 1852 Sir Edward Belcher was appointed to the command of an expedition to the Arctic Seas in search of the missing ships of Sir John Franklin. This expedition consisted of five vessels, and was separated into two divisions. The one proceeded westward to Melville Island, while Sir Edward, with two ships, ascended the Wellington Channel and wintered at its head, in an inlet which he named Northumberland Sound. In the spring of 1853 he personally explored by sledges to the north, discovered and partially surveyed North Cornwall, in latitude 77° 30′ N., and the strait bearing his name leading eastward into Jones' Sound; while other parties from his ships discovered and explored the north shores of Bathurst and Melville Islands, and, crossing the latter, communicated with the division of the squadron under Captain Kellett. A second winter was passed in Wellington Channel, and in the autumn of 1854, as there seemed no probability of extricating his ships, four of them were abandoned, and the crews returned over the ice to Beechey Island, whence they proceeded to England.

With this voyage Sir Edward Belcher's active professional career closed. He became a Fellow of our Society in December 1837, and died in London on March 18, 1877, in his seventy-eighth year. We are indebted to the *Proceedings of the Royal Geographical Society* for the information given above with regard to his many surveying voyages.

James Whatman Bosanquet was born on January 26, 1804. He was educated at Westminster School, and was taken into his father's banking house in 1822. He married, in 1840, the only daughter of the late Sir Nicholas Conyngham Tindal; in 1854, Frances, daughter of the late Lord John Somerset; and, in 1864, Emily, daughter of the late James Best, Esq., of Park House, Boxley.

His favourite study was Biblical Chronology. With the view of fixing dates for some of the facts mentioned in Hebrew and Assyrian history, he applied himself, with the assistance of the Astronomer Royal and Mr. Hind, to the study of early eclipses. Some of the principal dates verified by means of his researches are:

The date of the earthquake mentioned in Amos i. 1, which, it appears, took place on June 15, B.C. 763. The vision of Amos is described as taking place "two years before the earthquake," in the twenty-third year of Uzziah. This earthquake was identified by Mr. Bosanquet with an earthquake

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recorded in the Nineveh inscriptions, the record of which was discovered by Sir Henry Rawlinson. It is spoken of as one of a series of earthquakes which took place in the year of the eclipse of June, B.C. 763. The eclipse is believed to be referred to in Amos viii. 9, where it is said, "I will cause the sun to go down at noon, and I will darken the earth in the clear day."

The date of the fourteenth year of the reign of Hezekiah, which he identified as beginning in March, B.C. 690. This year was marked by an eclipse of the Sun on January 11, B.C. 689, which took place at noon-day. The eclipse Mr. Bosanquet believed was referred to in Isaiah xxxviii. 8.

The eclipse of Thales, in B.C. 585, he made use of to fix the date of the fall of Nineveh, B.C. 583, and the expulsion of the Scythians from Nineveh after their reign of twenty-eight years. The beginning of the reign of Nebuchadnezzar, and, consequently, the date of the captivity, which is coincident with the nineteenth year of Nebuchadnezzar, or B.C. 563.

Mr. Bosanquet was treasurer and took considerable interest in the establishment of the Society of Biblical Archæology. In a recent number of their publications it is mentioned that the Society was indebted to him for the payment of nearly one-half of the cost of their transactions, and that the late Mr. George Smith was first urged into publicity by his generosity.

Mr. Bosanquet was the author of several books and pamphlets, some of the more important of which are: *On the Date of the Fall of Sardis, and the Deposition of Crœsus; Assyrian and Hebrew Chronology Compared; Letter to the Right Hon. Sir George C. Lewis on the Bank Charter Act of* 1844; *On the Egyptian Dynasties of Manetho; On the Date of the Nativity of our Lord; Cyrus the Second; Date of the Fall of Nineveh; Chronological Remarks on the History of Esther and Ahasuerus.* Mr. Bosanquet died on December 22, 1877, at the age of 73.

James Scott Bowerbank was born in July 1797, in the parish of Bishopsgate, London, where his father was a rectifying distiller. He inherited a share of the business, and continued it, in conjunction with his brother, until 1847. In 1818 Dr. Bowerbank became a member of the old Mathematical Society of Spitalfields, and upon its incorporation in 1845 with the Royal Astronomical Society, he became a life member of the latter. He delivered several lectures before the Mathematical Society between the years 1822 and 1824 on Anatomy, Physiology, and Botany; and in 1831 he gave a course of lectures on Human Osteology. It is stated that he first acquired a love for natural history by studying the plants &c. growing in the river Lea, his father having reuted a portion of this river in the Edmonton Marshes. His first paper, "Observations upon the Circulation of the Blood in Insects," was published in the *Entomological*

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Magazine for 1833. He became deeply interested in the structure of flints, and arrived at the conviction that flints were formed by the siliceous fossilisation of the *Spongiadæ*, and it is in connection with researches upon sponges that his name will always be known. Of his papers on this subject, published in the *Transactions of the Geological Society* and elsewhere, it is unnecessary to speak, but his elaborate memoir on the "Anatomy and the Physiology of the *Spongiadæ*," printed in the *Philosophical Transactions* for 1858 and 1862, may be specially referred to.

Dr. Bowerbank was one of the founders of the Microscopical Society and of the Palæontological Society. Of the latter he was honorary secretary for many years. He was one of the earliest members of the Entomological Society, and one of the founders, and for many years treasurer, of the Ray Society. He was also a fellow of the Royal, the Linnean, the Geological, and the Chemical Societies; and he received the degree of LL.D.

Dr. Bowerbank's museum was well known on account of its extent and great value. The fossils in his collection were generally selected by the most eminent geologists of the day to illustrate their works; and of the fossil fruits and seeds of the London clay of Sheppey he possessed at one time not less than 100,000 specimens. In his later years he retired to St. Leonards-on-Sea, where he died at his residence on March 9, 1877, in the eightieth year of his age; he was buried in Hollington churchyard.

Mr. Julius Page, who was also a member of the old Mathematical Society, has kindly written the following notes with regard to Dr. Bowerbank's connection with that Society and other matters. Some account of the Mathematical Society was given in the fifty-fifth *Annual Report*, vol. xxxv., pp. 180–183 (1875), in the obituary notice of the late Mr. John Williams. Mr. Page writes:—

"It was about the year 1824 that I first joined the old Mathematical Society in Crispin Street, Spitalfields, and there made the acquaintance of the late Dr. J. S. Bowerbank. From that time I enjoyed his friendship and society until within the last three years, when my own failing health prevented my visiting him at St. Leonards, although our correspondence was kept up to the last.

"Formerly the Mathematical Society gave a course of public lectures during the winter months on the usual scientific subjects; but I do not believe any were given after the year 1821. The members were, however, expected to give lectures or demonstrations, each in his turn. Dr. Bowerbank was one of the most efficient of our members. His studies at that time were chiefly confined to Botany, Geology, and Anatomy. His botanical drawings and models, then employed for illustration, are still, I believe, in the possession of one of the hospitals, where they were formerly used in the lectures.

"Many able scientific men were members of the old Society, among whom I may mention Mr. John Crosley, who was ap-

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pointed by Dr. Maskelyne Assistant at the Royal Observatory, Greenwich. Among the members of my own time were Benjamin Gompertz, Mark Noble, W. Woodall, and J. J. Downes, all known in their day as mathematicians. When I became a member, the Rev. Mr. Parvissien, of Hackney Church, was the president, and he was succeeded by Mr. Benjamin Gompertz; Mr. John Williams was elected secretary in or about 1826.

"The Mathematical Society was founded in 1717, and when I joined it the number of members was, by the rules, restricted to 81; but some years after it gradually fell off, until, in 1845, the number became reduced to about 18. It was then proposed that we should give up our library, which contained many curious and valuable books, to the Royal Astronomical Society, but should retain the right of consulting it. This was acceded to, and we were elected life members of the Royal Astronomical Society on June 13, 1845.

"The late Professor De Morgan, in his *Budget of Paradoxes*, gives an amusing account of the old Mathematical Society. He had been deputed by the Astronomical Society to inspect us, and to report whether we were the kind of persons that the Astronomical Society could admit. His account being favourable, we were admitted. His description of our Society is very graphic, and is worth reading.

"The old Mathematical Society did good service in its time, and to my knowledge several young persons acquired an inclination to study who would probably have otherwise never had their thoughts turned in this direction. The history of the Society would be interesting, but the only man who could have done justice to it was the late Mr. John Williams, the Secretary, who was afterwards Assistant Secretary of the Astronomical and Microscopical Societies; but he died in 1874. All the old records and books of account of the Mathematical Society are in the possession of the Royal Astronomical Society.

"When I first began to visit Dr. Bowerbank, he was living in the New North Road, where I often worked with him in mounting specimens for the microscope. Soon after he removed to Park Street, Islington, where he had much larger rooms and was able to show his treasures; it was here that his celebrated Monday soirées were commenced. But his natural history collections increased, and in 1846 he built his museum at Highbury, in which he had a still larger room for his specimens and also more space to receive his visitors, who were now very numerous. Finding afterwards that it was necessary on account of Mrs. Bowerbank's health and his own to remove to the seaside, he finally left London and established himself at St. Leonards, where he died. I have mentioned his different removals in order to trace the origin and progress of his Monday evening meetings, which were much appreciated by the scientific world.

"It was in 1841, when we were together at Brighton, that occurred the fortunate opportunity which directed all his future

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studies of the sponges. There had been a severe storm and there was left along the beach a thick ridge of sponges, seaweed, &c. He collected a great many of the most promising specimens of the sponges and took them away with him in glass jars. It is needless to remark that he derived more information from them than from many times their number of dried specimens. He had several agents on the coast who collected and forwarded to him specimens of fossils washed out of the cliff, and he now engaged them and others to forward him sponges recently cast up. Through the agency of friends he also obtained many large consignments of sponges from distant parts of the world. His instructions to his agents were always to only remove as much of the watery matter as would prevent the specimen from rotting, and not to send clean washed-out sponges as if intended for the bath-room. He had a large quantity of sponges packed in cases, many of which he had examined under the microscope."

Olinthus Gregory Downes, second son of James John Downes, was born in London on April 7, 1823. From his earliest youth he displayed a taste for scientific pursuits, coupled with mechanical ingenuity, and while still almost a boy joined the Islington Literary Society, where he delivered many interesting and instructive lectures. In 1840 he became a clerk at the Economic Life Assurance Society, where his father, whom he succeeded in 1862, was filling the post of actuary. In 1849 he translated Quetelet's *Lettres sur la Théorie des Probabilités*, and the work was noticed by Sir John Herschel in the *Edinburgh Review* for July 1850. In 1860 he published his book upon the physical constitution of comets, which was noticed in the *Monthly Notices*, vol. xx., p. 59. In 1850 he was elected a Fellow of this Society, of which his father and elder brothers were also Fellows (obituary notices of them appeared in vol. xxviii., p. 76, and vol. xxix., p. 122). He was also a member of the London Mathematical Society. In 1869 he resigned his position as actuary of the Economic, and lived in retirement in the country until the time of his decease in May 1877. In December 1855 he married Anne, eldest daughter of the late Colonel Stewart, 46th Regiment. His integrity and talents gained him many friends when in active life who now deeply regret his loss. He has left a widow and four children.

The Rev. William Lake Onslow was the eldest son of Captain John James Onslow, Post Captain in the Royal Navy, and grandson of Admiral Sir Richard Onslow, Bart., G.C.B. He was born at Bishop Wearmouth, in the county of Durham, on May 10, 1820, and received his preliminary education at the Grammar School, Yarmouth. Afterwards he was sent to Emmanuel College, Cambridge, where he obtained an exhibition, and took his degree

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with mathematical honours in 1842. While at college he pulled bow oar in the University eight in two successful encounters with the Oxford crew. In 1844 he was ordained by the Bishop of Norwich, and for some time served the cures of Witton and Brundell, near Norwich. In 1846 he received an appointment as chaplain and naval instructor on board H.M.S. *Hastings*, and left for service on the coasts of South America, India, and China. Afterwards he served in the Baltic, the Mediterranean, and the Crimea, and was successively transferred to H.M.S. *Carysford*, *Duke of Wellington*, *Diadem*, *Immortalité*, and *Racoon*.

In 1866 he was nominated special naval instructor to H.R.H. Prince Alfred on board H.M.S. *Euryalus*, his last service afloat being in the *Ariadne*, in 1868–69, during the visit of their Royal Highnesses the Prince and Princess of Wales to the East, after which he was placed on the retired list, and was appointed rector of Sandringham-cum-Babingley and private chaplain in ordinary to H.R.H. the Prince of Wales, which position he occupied till his death on August 28, 1877.

Rev. Frederic William Russell was born at Norwich on January 30, 1821, and received his early education under the Rev. Charles Turner. He entered University College, Durham, where he took the degree of M.A., in the usual course, in 1847, and four years after was elected a Fellow of this Society. For some years he was arduously engaged in parochial work; but in 1862 he was appointed Chaplain of Charing Cross Hospital. Here he laboured diligently for fifteen years, and doubtless overtaxed his strength. For some time before his death he appeared to be failing, but still kept to his work. He was, however, taken ill in the hospital, and died there on April 9, 1877, after a few days' illness. In 1857 he married Fanny, youngest daughter of Benjamin Standring, Esq., a London merchant. He was Fellow of the Society of Antiquaries, and in 1859 published a work entitled *Kett's Rebellion in Norfolk*. He was much interested in scientific matters, especially in the microscope, and was a Fellow of the Royal Microscopical Society. He has left a widow and six children.

William Henry Fox Talbot, of Lacock Abbey, Wiltshire, was the eldest son of the late Mr. William Davenport Talbot by his marriage with Lady Elizabeth Theresa Fox-Strangeways, eldest daughter of Henry Thomas, second Earl of Ilchester, and was born in February 1800. He received his early education at Harrow School, and afterwards proceeded to Trinity College, Cambridge. He graduated in 1821 as twelfth wrangler, and obtained the second Chancellor's Medal. In 1820 he had received the Porson Prize for Greek Iambics, which had been founded three years previously. His earliest papers were mathe-

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matical, the first being an extract from a letter addressed to Gergonne containing a solution of a problem which had appeared in his journal: it is printed in vol. xiii. of *Gergonne's Annales* (1822). He seems to have been chiefly interested in theorems involving arcs of curves, of which Fagnani's theorem is the type. Mr. Fox Talbot's name is so completely associated with the invention of photography that his mathematical powers are now almost unknown except to the few mathematicians who have attended to the subject of elliptic functions. In his memoir, "Researches in the Integral Calculus," published in the *Philosophical Transactions for* 1836 and 1837, he gave an account of his investigations upon the comparison of transcendents, which show that he had independently been led to consider the development and generalisation of Fagnani's theorem, and was on the track that might have led him to rediscover Abel's great theorem. As it is, although Mr. Talbot's work was in fact superseded before it was published by the more brilliant discoveries of the Norwegian mathematician, still, had it been published twenty years earlier, it would have made an important extension of the boundaries of mathematical science, and it is probable that Mr. Talbot's researches upon the comparison of transcendents more nearly approached those of Abel than had those of any other mathematician. In later years Mr. Talbot returned to the subject of mathematics, but his recent papers do not appear to be of much importance.

With regard to the invention of photography, Mr. Talbot himself states, in his *Pencil of Nature*, that in the month of October 1833, when trying to sketch the scenery along the shores of the Lake of Como by the aid of a camera-lucida, and wearied by many successive failures, he was "led to reflect on the inimitable beauty of the pictures painted by the hand of Nature, pictures which the glass lens of the camera throws upon the paper in its focus," and, further, "to consider whether it would be possible to make these pictures permanent." By a long and elaborate course of experiments, extending over five years, he had nearly arrived at a result satisfactory to himself, when he read one day, in a scientific journal, that his own solution of the mystery had been, if not anticipated, at all events rivalled by the parallel researches of M. Daguerre. M. Daguerre's account of what was subsequently termed the Daguerreotype process was published in January 1839, and Mr. Fox Talbot lost no time in communicating to the Royal Society, on January 31, 1839, an account of his researches, in a paper entitled "Some account of the Art of Photogenic Drawing; or, the process by which natural objects may be made to delineate themselves without the aid of the artist's pencil," which was published in the *Proceedings* and also in the *Philosophical Magazine* for 1839. The process consisted in treating writing-paper with a solution of common salt, and subsequently with a solution of nitrate of silver, forming chloride of silver on the surface of the paper.

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Lace, leaves, ferns, &c. were laid upon this paper, and after exposure to the light, produced white images of the objects upon a dark ground: these were the negatives, from which positives could be obtained by printing in the manner still employed.

In 1840 Mr. Talbot discovered the Calotype process, the principle of which was that paper rendered sensitive by iodide of silver and nitrate of silver receives, in the first few seconds of its exposure to the light, an invisible image, which can be rendered visible by treating it with a solution of gallic acid.

This Calotype method, which was subsequently named Talbotype, after its inventor, may be regarded as the foundation of the photography of the present day. It formed a complete and perfect process and was largely practised by the early photographic amateurs, some of whom by means of it produced almost as perfect results as have been obtained by the methods invented subsequently. The *Pencil of Nature* referred to above is a fine quarto volume, published in 1844, and is without doubt the first book ever illustrated by photographs. It contains twenty-four photographs, each picture having a descriptive chapter of letter press, and an introduction giving an outline of the process employed and the steps which led to its discovery. Subsequently Mr. Talbot discovered a method by which instantaneous pictures could be taken, and a method of photographic engraving. All these inventions were patented; but in 1852, at the solicitation of the Presidents of the Royal Society and Royal Academy, he consented to throw open his discovery to the world for the sake of advancing the art, with the sole exception of "photograph-taking for sale to the public."

Mr. Talbot also wrote numerous papers upon such various subjects as light, spectrum analysis, heat, electricity, crystallography, and even botany.

In 1842 he read, at the British Association, a paper "On the Improvement of the Telescope," and in the British Association Report for 1871 there is a short paper by him "On a New Method of Estimating the Distance of some of the Fixed Stars." He was also one of the first who, with Sir Henry Rawlinson and Dr. Hincks, deciphered the cuneiform inscriptions brought from Nineveh, and he made numerous contributions in literature and archæology to the Royal Society of Literature and to the Society of Biblical Archæology. Besides the *Pencil of Nature*, he published *Legendary Tales; Hermes, or Classical and Antiquarian Researches; The Antiquity of the Book of Genesis illustrated by some New Arguments;* and *English Etymologies.*

Mr. Talbot was one of the oldest members of this Society, having been elected on December 13, 1822. He was elected a Fellow of the Royal Society in 1831, and received a Royal Medal in 1838 and the Rumford Medal in 1842. He sat in the first reformed parliament for two years, for Chippenham, but then retired from politics. In 1832 he married Constance, youngest daughter of the late Mr. Francis Mundy, of Mark-

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eaton, Derbyshire, by whom he has left a family. His death occurred on September 17, 1877, at Lacock Abbey.

CARL BREMIKER was born on February 23, 1804, at Hagen in the county of Mark. He at first was educated as a surveyor, and was engaged up to 1835 on the Rhenish-Westphalian survey. In 1835 he went to Berlin in order to devote himself to mathematical and astronomical studies, and with great success, as appears from the fact that he performed the calculations for the important appearance of Encke's Comet in 1838 with such care and accuracy that Encke asserted that more could not have been done. After Bremiker had graduated at the university he became a *collaborateur* on the *Berliner Jahrbuch*, and afterwards Inspector of the Plan Room in the Prussian Ministry of Trade. He proved his high interest in practical astronomy by the observations, continued during many years, that he made at his own residence. As a result of his labours should be mentioned his discovery of a comet on October 26, 1840, for whose orbit the observations gave a period of 344 years with great probability. He also there made the revisions and entries for five of the star charts of the Berlin Academy, which, with the corresponding catalogues, were presented to the Academy: Hora xvii. on December 9, 1839, Hora xiii. on December 6, 1841, Hora xxi. on December 9, 1844, Hora vi. at the end of 1852, and Hora ix. on May 26, 1858. It is well known that, in September 1846, the existence of a new planet beyond *Uranus*, announced by Leverrier, was at once verified by means of Bremiker's Star Chart, Hora xxi., which was lying for correction at the Berlin Observatory.

Bremiker performed a great service by the publication of tables of logarithms arranged in an extremely practical form, and which have found much favour. His *Logarithmorum* vi *decimalium Nova Tabula Berolinensis* appeared in 1852. In the very valuable introduction of 82 pages there are many important remarks upon the art of exact numerical calculation. In 1856 he issued a corresponding edition of Vega's seven-figure Table, and he also published subsequently four-figure and three-figure tables of logarithms, and finally, in 1872, a five-figure table of logarithms of trigonometrical functions, the degree being divided not into minutes, but into hundredths of a degree, to which are added Gaussian logarithms, a table of squares, &c.

He also published a new and remodelled edition of Crelle's well-known *Rechentafeln*, the whole table being contained in one volume, and in 1843 appeared his *Tafel der Proportionaltheile zum Gebrauche bei logarithmischen Rechnungen.* Since 1850 he edited the *Nautische Jahrbuch* for the use of the German Marine.

In 1868 Bremiker accepted the position of Departmental Director in the Royal Prussian Geodetical Institute, and he has consequently, in the last ten years, taken a very active part in

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the works in connection with geodesy in Germany. He has also published some detailed theoretical investigations on this subject, viz. "Ueber die Projection der Land- und Himmelskarten," 1855 (*Astronomische Nachrichten*, No. 983); "Ueber Gradmessungen," 1856 (*Astronomische Nachrichten*, No. 1022); "Studien über Höhere Geodäsie" (1869).

Bremiker died, after a short illness, on March 26, 1877. He was elected an Associate of our Society on May 12, 1848. For the above account of Dr. Bremiker's life and works we are indebted to the kindness of Dr. Winnecke, Director of the Strassburg Observatory.

Eduard Heis was a native of Cologne, where he was born on the 18th of February 1806. He completed his education at the University of Bonn in 1827, in which year he became a teacher of mathematics and physics in the Friedrich Wilhelm Gymnasium at Cologne, where he resided during the succeeding ten years. His talents as a teacher having become known to the authorities of the High School at Aix-la-Chapelle, he was induced, in the year 1837, to accept the position of principal master for mathematics, physics, and chemistry in that school. Here he remained till 1852, to the advantage of those who came under his instruction, and his careful method of teaching made him popular among his pupils. It was while fulfilling his duties at this establishment that his acuteness of vision led him to undertake that long series of records of the relative brightness of the stars, as viewed with the unassisted eye, which formed the materials for that excellent and standard catalogue of stellar magnitudes contained in his *Atlas Cœlestis Novus*. During this period he also found ample opportunities for observing various other occasional celestial phenomena, especially of the zodiacal light, and of casual meteors, bolides, or the periodic recurrences of meteor showers. In 1852 Dr. Heis was transferred to Münster, in Westphalia, having received the appointment of Professor of Mathematics and Astronomy in the Royal Academy of that city. This office, in conjunction with that of Director of the Observatory, he retained to the end of his life.

Dr. Heis became known as a scientific writer soon after he completed his studies at the Bonn University. He employed his leisure hours between his ordinary scholastic duties at the Cologne Gymnasium chiefly in the study of botany, and his first published paper in 1828 was devoted to a botanical investigation "Ueber die Structur der Pflanzenzellen." Other papers relating to the same branch of science followed, and for some time this was evidently his chief recreative pursuit. He may possibly have taken an occasional interest in the appearance of irregular celestial phenomena, but we can find no record before 1833 bearing even slightly on his future astronomical eye-observations; and it is tolerably certain that at this time his thoughts were

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little occupied on astronomy as a subject of research. In 1833 he commenced his notes on the appearance and direction of motion of shooting stars. It may be interesting to remark that his first recorded observation of a meteor was made on October 2 of that year, at seven in the evening, when he noticed, from Steinheim, near Hanau, a fireball appear near *α Andromedæ*, passing through *Cygnus* to near *Vega*, where it exploded. His second recorded observation was not made till August 10, 1839, between which date and November 1847 occasional meteors only were observed by him, a few in almost every year. In 1847 Dr. Heis published his first paper on the meteors observed to that date, and this has been followed by several others at frequent intervals in connexion with meteoric astronomy. Before his sudden death Dr. Heis had collected all his meteor-observations from 1833 to 1875, and arranged them in a convenient form for publication in one volume, forming vol. ii. of the *Publications of the Royal Observatory at Münster*, and the revision of the greater number of the proof-sheets was made by him before they were passed for press. This valuable work, *Resultate der in den 43 Jahren* 1833–1875 *angestellten Sternschnuppen Beobachtungen*, contains the right ascension and declination of the points in the heavens of the beginning and ending of the track of each meteor, which on some days amounted to several hundreds. No one more than he took greater pleasure in recording any special observations of meteors, especially of the periodical showers belonging to the principal August and November streams, the full details of which are contained in this volume. Dr. Heis was also an assiduous observer of the aurora borealis and the zodiacal light, and he never willingly lost an opportunity of increasing the number of observed facts relating to these phenomena at successive appearances. The results of his observations and investigations on these subjects have been published from time to time in various scientific serials; but those on the zodiacal light have been collected and discussed by Dr. Heis in the first volume of his Observatory publications, which appeared in the year 1875.

Professor Heis has at various times paid particular attention to the variability in the magnitude of the larger periodic stars; among these *Algol*, *Mira Ceti*, and *χ Cygni* were favourite objects of observation. Their periodical variations of magnitude were carefully noted by him, and his deduced periods of variability of these stars are considered to be most trustworthy. He was indebted to his friend Professor Argelander, whose experience in the observation of stellar magnitudes was perhaps greater than that of any other astronomer, for some valuable suggestions as to the best method of making the comparisons of magnitude on different days, and which he advantageously adopted in his observations.

But the work which will connect the name of Professor Heis with the history of astronomy of the present century is un-

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doubtedly his *Atlas Cœlestis Novus*, consisting of twelve maps, accompanied by an index-catalogue arranged according to the constellations, the result of a twenty-seven years' continuous examination of the relative magnitudes of nearly all the stars visible to the naked eye from the latitude of Münster. In presenting these observations, Heis has evidently used Argelander's *Uranometria* as his working catalogue, as he has adopted the same form and arrangement of the results, and in some cases he has apparently been influenced by the previous magnitudes of Argelander; but though in general the corresponding magnitudes in the two works are in agreement, there is every probability that Heis's values as a whole are independently estimated, for he remarks that the magnitudes in the *Atlas Cœlestis Novus* were derived from an independent method devised by himself, and that he endeavoured on all occasions to avoid any possible influence from the magnitudes of stars previously published. In the Atlas the varying luminosity of the Milky Way is represented in different shades with a good effect, but the insertion of the figures of the constellations in addition to the boundaries is rather distracting to the eye, and for this reason it would have been better had they been omitted. Dr. Heis's unusually acute vision enabled him to see smaller stars with the unassisted eye than most ordinary observers, and it is rather remarkable that he should have been able to note the magnitudes of 2,153 stars which had been passed over by Argelander, of which 1,821 are smaller and—what is more remarkable—34 greater than the sixth magnitude. The whole number of stars contained in the catalogue whose magnitudes are recorded is 5,421. The great excess of stars in the *Atlas Cœlestis Novus* over those in the *Uranometria*, especially of the $6\frac{1}{2}$ magnitude, gives to the former a value peculiar to itself. It appeared in 1872 as an atlas in folio, with the index-catalogue in octavo. Its publication brought Dr. Heis's name prominently before astronomers out of Germany, and attention was thus more directed to this hard-working professor, whose continuous labours had hitherto only been appreciated by those who took a common interest with him in the observation of meteors, the aurora borealis, the zodiacal light, and other occasional astronomical phenomena. On January 9, 1874, his important services were recognised by the Royal Astronomical Society by electing him an Associate.

Professor Heis was the editor of a German astronomical periodical entitled *Wochenschrift für Astronomie*, which, though well-known in Germany, had only a limited circulation out of that country. Several of his contributions to meteoric astronomy and other subjects have appeared in that journal, which accounts for so few of them having come under the eyes of English astronomers. Some of his most valuable papers may, however, be found in the *Astronomische Nachrichten*, the *Bulletins de l'Académie Royale de Bruxelles*, and a few other serial publications which are generally available for reference. His sole

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contribution to the *Monthly Notices* is a list of radiant points of meteors observed within the eleven years preceding 1860. He is also the author of a popular collection of examples and problems in general arithmetic and algebra, which has passed through a considerable number of editions in Germany.

Professor Heis remained an active worker in our science to the last, and it has been already remarked that at the time of his death he was engaged in passing his collection of meteor observations through the press. He died suddenly of apoplexy on the 30th of June 1877, in the seventy-second year of his age. His name will remain honoured as representing a zealous, hard-working, and careful observer, who, regardless of personal inconvenience, has accumulated a vast store of results of great practical utility to his fellow-observers, as well as to the science of astronomy generally. E. D.

URBAIN JEAN JOSEPH LE VERRIER was born on March 11, 1811, in the department of the Manche. Of his earlier education we have no record; but at twenty years of age he joined the Ecole Polytechnique at Paris, where he studied two years, and on leaving the institution in 1833 had attained a rank which entitled him to make choice of one of the higher public services, and he accordingly became attached by preference to the "Administration des Tabacs," as engineer. In this position his duties also involved employment in the laboratory, and it thus probably arose that the first scientific papers published in his name were upon chemical subjects, viz. "Sur les combinaisons du phosphore avec l'hydrogène" and "Sur les combinaisons du phosphore avec l'oxigène," which appeared in the *Annales de Chimie* in 1836 and 1837 respectively. Notwithstanding, however, that Le Verrier's earlier memoirs were upon subjects connected with a department of science at that time distinct from astronomy, it is evident, from a very interesting note communicated by Sir George Airy to M. Tresca a few months since, that Le Verrier's attention had been directed to the particular science in which he has left so illustrious a name previously to his joining the "Administration des Tabacs" —or during his course of study at the École Polytechnique. Sir George states that he was probably Le Verrier's oldest scientific friend—that he believes his name had been known to him since the year 1832, about which time the Astronomer Royal's researches on the motions of the Earth and *Venus* were made public.

Le Verrier's first astronomical memoir was presented to the Academy of Sciences on September 16, 1839. It is entitled "On the Secular Variations of the Elements of the Orbits of the Seven Principal Planets," and was printed *in extenso* in the additions to the *Connaissance des Temps* for 1843, which appeared in August 1840. Thus Le Verrier's first memoir has reference to one of the most interesting and important questions in astronomy,

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the stability of the planetary system. Lagrange and Laplace had shown that the mean distances of the planets from the Sun are invariable. With regard to the eccentricities and inclinations it had been shown by Laplace that neither would increase indefinitely with the time, though without indication of the limits within which their variations must be confined. The first attempt in this direction was made by Lagrange, as regards the elements of *Jupiter* and *Saturn* in 1776, and for the four smaller planets a few years later; but the values of the masses employed in his researches, particularly of the mass of *Venus*, were open to considerable corrections, and hence a degree of uncertainty attached to his inferences. Le Verrier undertook the long and difficult numerical calculation still necessary to decide whether the elements of the planetary system are liable only to changes of moderate magnitude about a mean state, and in the investigation in question he takes into account the simultaneous action of the seven principal planets. The first part of the memoir treats upon the eccentricities and apsides, one of the most interesting results referring to the eccentricity of the Earth's orbit, which it is found will diminish during nearly 24,000 years, attaining then a minimum equal to 0·0033; it then again increases until some 70,000 years hence a maximum of about 0·021 is attained, though Le Verrier finds that the highest value the Earth's eccentricity can reach is 0·07775. *Jupiter*, *Saturn*, and *Uranus* he found to compose a system independent of the other planets, the eccentricities and the lines of apsides returning to the same condition after a lapse of 900,000 years. The maxima and minima of the eccentricity of *Saturn* he found to be separated by an interval of 16,114 years. The change in the eccentricity of *Mars* is exceedingly slow, a period of 1,800,000 years being required for the exhibition of the most pronounced maxima. Le Verrier then treats of the secular variations of the inclinations and longitudes of the nodes, and assigns the limits of the inclinations upon the ecliptic of 1800 and also the limits of the relative inclinations of the planetary orbits. Finally he appends tables showing the elements of *Mercury*, *Venus*, the Earth, and *Mars* for a period of 200,000 years, or for every 10,000 years from −100,000 to +100,000 counted from 1800. It should be noted that the superior limits of the eccentricities of these four planets assigned by Le Verrier are very similar to the results obtained by Lagrange, an agreement which arises from the fact, pointed out by Le Verrier, that this higher limit will change only to a trifling extent, though considerable variation may be introduced in the values of the adopted masses.

A year later Le Verrier returned to this subject in a paper printed in the *Connaissance des Temps* for 1844, in which the approximations are carried further than in the previous memoir. The stability of the system formed by *Jupiter*, *Saturn*, and *Uranus* is confirmed, but the method of successive approximations is shown to be affected in its application to *Mercury*, *Venus*, the

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Earth, and *Mars* by uncertainty still remaining in the values of their masses. Le Verrier leaves his investigation in such manner that, when the definitive corrections to the masses he has employed are known, a comparatively simple calculation will lead to the corresponding corrected results without the necessity of repeating the laborious numerical computations previously involved.

In 1843 Le Verrier presented to the Paris Academy a "Theory of the Motion of *Mercury*," which was subsequently handled more completely, and definitely finished in 1859. The first part contains the calculation of the perturbations of the heliocentric motion of the planet; in the second part the theory thus formed is compared with meridian observations made at Paris from 1801 to 1842, and with the whole series of observed transits of *Mercury*, which are thoroughly reduced and discussed. In this section he determines the diameter of the Sun from the durations of the transits of *Mercury* across his disk. The third part relates to the construction of the usual tables for the calculation of the planet's geocentric positions; tables in which he intended to introduce modifications from previous forms calculated to greatly abridge the preparation of ephemerides.

During several following years Le Verrier's attention was more or less directed to the motions of the comets of short period, particularly that of 1770, commonly termed Lexell's Comet, and those of Faye and De Vico, discovered in the years 1843 and 1844 respectively. The amount of labour and research involved in these investigations, more especially in that relating to the comet of 1770, was enormous, and their publication excited a high degree of interest amongst astronomers.

The comet of 1770 was discovered by Messier on June 15, and approached the Earth until on July 1 it was distant only 363 terrestrial semi-diameters. It was observed until the beginning of October, and thus a wide extent of observation was afforded for the determination of the orbit. Pingré, Prosperin, Lambert, and others failed to represent the comet's path by a parabolic orbit, and to Lexell was due the merit of having first determined its true form—an ellipse of very limited dimensions, the period of revolution, while the comet was visible, being only five and a half years. Lexell further explained a circumstance which at first excited surprise, that a comet revolving in so short a period should have been observed at a single appearance only. He remarked that the comet must have passed so near to the planet *Jupiter* in May 1767 that its influence would be considerably greater than that of the Sun, and hence he thought the form of orbit in which the comet was moving in 1770 might have been impressed upon it as late as 1767. And he further accounted for its non-appearance since 1770 by the near approach which the comet might again make to *Jupiter* in the summer of the year 1779. In 1806 Burckhardt, at the instance of Laplace, verified Lexell's calculations, and further applying formulæ

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investigated by the French mathematician, he determined the actual effect of the two near approximations to the planet upon the comet's orbit. Burckhardt's results were published in the *Mécanique Céleste*, and were received with confidence by astronomers generally.

In this state the subject remained until it was taken up by Le Verrier. The principal results of his researches were communicated to the Paris Academy in 1844 and 1848, but the details were not fully published until the year 1857, when they appeared in the third volume of *Annales* of the Observatory at Paris. He found, from a minute discussion of the observations, that the mean motion of the comet could not be precisely determined in 1770, the observations leaving a degree of uncertainty as to the exact value of this important element which rendered the calculation of the comet's subsequent path to some extent doubtful, and thus it became necessary to consider the possible limits of uncertainty of elements in the investigation of the effect of the near approach of the comet to *Jupiter* in 1779. Following the comet in this way through the several orbits in which it might have moved after its disappearance in 1770, Le Verrier found that it is impossible it could have been retained in the system of *Jupiter* so as to have become a satellite of that planet: it is not impossible that after the great perturbation in 1779 its orbit may have become a hyperbola, while, on the contrary, it may have continued an ellipse, or rather, he showed that there were many possible elliptical trajectories. And finally he tabulates all these possible ellipses, so that, if a new periodical comet were detected, on examining the course previously followed by it, and comparing with the possible tracks of the comet of 1770, it should be practicable to pronounce upon the identity of the bodies or otherwise. Le Verrier also ascertained that Burckhardt's calculations for the year 1767, in the *Mécanique Céleste*, were vitiated by errors, the cause of which has since been explained by D'Arrest.

The comet of Faye, discovered on November 22, 1843, which was soon found to be moving in an elliptical orbit, with short period, was also made the subject of an immense work by Le Verrier, who had to some extent favoured the idea of its identity with the comet of 1770. He did not shrink from the laborious calculation of the effect of planetary perturbations from 1843 to 1781, the result of which negatived the conjectured identity. Continuing his researches still further backwards, he ultimately came to the conclusion that "it is possible the comet of Faye, in the year 1747, may have passed so near to *Jupiter* that its path was completely changed: it is at least to this year that it is necessary to remount to fix an epoch when the comet commenced describing the restricted ellipse in which we have observed it in our days. It was not until its thirteenth return to perihelion that it was at last seized by M. Faye."

Similarly, the comet of De Vico, detected on August 22, 1844,

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engaged the attention of Le Verrier. Remarking that, like Faye's comet, it offered vague resemblance to the comet of 1770, and that, from the circumstances under which the comet near aphelion would approach the orbit of *Jupiter*, the large difference in the line of nodes did not discourage investigation, he proceeded to calculate the perturbations previous to 1844, starting from a system of elements obtained by Professor Brünnow from a very complete discussion of the observations in that year. The first case of great perturbation was found to occur in 1814, and after a careful study of their effect, it was found necessary to include the uncertainty of the previous elements in continuing the research to a more distant period. A table is presented, showing the elements for various epochs, as far back as 1753, on the supposition that the comet underwent the greatest possible amount of perturbation. As bearing upon the conjectured identity of this comet with the comet of 1770, Le Verrier found that between 1844 and 1787 the line of nodes would have retrograded 109°, so that perturbation sufficed to explain the great difference in this element; but he also ascertained that in other elements the differences were so great as to be incompatible with the hypothesis of identity, and thus he adds: "The two comets of Faye and De Vico, amongst which we had hoped to find again the comet of 1770, have in the result disappointed our expectations." Upon further considerations, Le Verrier inferred that the comet of De Vico had been a denizen of the planetary system for many centuries prior to its discovery in 1844, and he was thus led to seek for the comet amongst those of comparatively early date. The comet of 1585, observed by Tycho Brahe and Rothmann with a degree of precision very great for the epoch, and the elements of which presented some similarity to those of De Vico's comet, was first examined. Laugier and Mauvais had obtained for the comet of 1585 an ellipse of short period. Le Verrier concluded that there is no plausible reason for admitting the identity of the comets of 1585 and 1844; and, not satisfied with this inference, from a consideration of the relative position of the line of apsides, he showed that it is possible to represent the observations of 1585 within their probable limits of error by a parabolic orbit, a result which was verified by an independent investigation by Professor Peters, of Kiel.

The case is entirely different with the comet of 1678, observed only by La Hire at Paris, which in several of the elements of its orbit presented some degree of similarity to the comet of De Vico. Le Verrier examined if it were possible the perturbations of the latter would admit of its passing through the four positions observed by La Hire, and he found a system of elements from a consideration of the perturbations which, with trifling variation—the semi-axis remaining unchanged—sufficiently well represented the observed positions, and thus he decides in favour of the identity of the comets of 1678 and 1844. He concludes the account of his researches on the motion of De Vico's comet thus: "The

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comet of 1844 has, like others, come to us from the most distant regions of space, and has been fixed amongst the planets under the powerful influence of the action of *Jupiter*. Its advent ante-dates, without doubt, many centuries. Since that epoch it has often passed in the neighbourhood of the Earth, but has only been once observed in past centuries, 166 years before its appearance in 1844." He considered that the comet will describe the orbit in which we have observed it for a long time to come, and that, after a certain number of centuries, it will again attain the orbit of *Jupiter* in a direction opposite to that from which it arrived in the planetary system, and its course will again be wholly changed.

It was while he was engaged upon these researches relating to the comets of short period that Le Verrier's attention was first specially directed to the anomalous motion of the planet *Uranus*. In the formation of the Tables of *Uranus*, published in 1821, Bouvard had experienced unforeseen difficulties in reconciling the older observations of the planet with the modern ones. If the early observations were combined with the later series, while the first were passably represented, the latter showed differences much exceeding the admissible errors of observation; while, if the ancient observations were omitted from the discussion, the modern series could be represented satisfactorily, but at the expense of large differences, when the theory was compared with the early observations of Flamsteed, Bradley, Mayer, and Lemonnier. Under these circumstances, Bouvard decided to found his tables upon the modern observations exclusively, leaving, as he says, to a future time the decision whether the difficulties he had encountered were attributable to inaccuracies in the older observations, or whether they arose from some extraneous action which had been exercised upon *Uranus*. Writing to the Astronomer Royal in October 1837, Eugène Bouvard, the nephew of the author of the Tables of *Uranus*, mentions that the latter had deputed to him their reconstruction; he referred to the constantly increasing differences between calculation and observation, and suggests whether they could be owing to the action of a body situated beyond the planet. Such, at least, he states to have been the opinion of the elder Bouvard. Again, in 1840, Bessel, in a letter addressed to Humboldt, expresses the opinion that a time would come when "the mystery of *Uranus*" would be solved by a new planet, the elements of which would be recognised from its action upon *Uranus*, and verified by that which it might also exercise upon *Saturn*.

These circumstances would, no doubt, be well known to Arago; and accordingly, in the summer of 1845, we find him representing to Le Verrier the importance of the question, and the duty which it imposed upon every astronomer to cooperate towards clearing up the difficulty. In his *Première Mémoire sur la théorie d'Uranus*, presented to the Paris Academy on November 10, 1845, Le Verrier writes: "I therefore abandoned for the

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moment, to occupy myself upon *Uranus*, the researches which I had undertaken relating to the comets, of which many fragments have already appeared. Such is the origin of the work which I have the honour to present to the Academy to-day." This distinct acknowledgment that it was Arago who first engaged him to study the theory of *Uranus* will be found in the *Comptes Rendus* for the above date.

In this first memoir the perturbations produced by *Jupiter* and *Saturn* were newly and completely developed and calculated, the most important numbers being verified by a duplicate process, involving entirely different methods. Le Verrier found corrections which materially affected the places deduced from the Tables of Bouvard, but which nevertheless failed to reconcile the observed and computed positions. This work was one of great labour, and as the consequence of it, to quote the words of the Astronomer Royal: "Perhaps it may be truly said, that the theory of *Uranus* was now, for the first time, placed on a satisfactory foundation."

The second memoir was presented to the Academy on June 1, 1846. Le Verrier first reduced anew nearly all the reliable existing observations of *Uranus*, and compared them with the theory established in his first memoir, and he arrived at the definite conclusion that it was impossible to represent the observed motion of *Uranus* without admitting the presence of some extraneous action. The failure of the law of gravitation at the distance of *Uranus*, which had been already suggested, he at once rejected, remarking that every suspicion of the kind had but served to afford a further confirmation of the universality of the Newtonian law; and, dismissing other explanations that offered themselves as insufficient or inapplicable, he is led to the consideration of the problem involved in the following question: "Is it possible that the irregularities of *Uranus* can be due to the action of a planet situate in the ecliptic at a distance double that of *Uranus*? And if so, where is that planet actually situated? What is its mass? What are the elements of the orbit which it describes?" The problem thus distinctly announced, he proceeded, as he states, to solve rigorously. He found that there was but one region of the ecliptic where a disturbing body could be placed so as to satisfy the observed irregularities in the motion of *Uranus*, and concluded that on January 1, 1847, its heliocentric longitude must be very nearly 325°. This he announced as the capital result of his researches, adding that, to arrive at it, he had been under the necessity of undertaking the calculation of the perturbations exercised by *Jupiter* upon *Uranus*, determining also those due to *Saturn*, carrying the approximations to the squares and products of the masses, which had introduced notable modifications in the existing or admitted theories; of reducing nearly three hundred meridian observations of *Uranus*; of calculating the corresponding heliocentric positions of the planet, supposing that it was

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subject only to the action of the Sun, *Jupiter*, and *Saturn*, and of deducing the geocentric coordinates with the help of the Solar Tables, thus proving the absolute incompatibility between the places calculated and those observed. The existence of an undiscovered perturbing body being thus indicated, there remained, in order to assign its place in the heavens, the inverse solution of the problem of perturbations in its usual form, or the investigation of the elements of the unseen body from its measured effect upon the motion of *Uranus*. Recalling a remark of Clairaut's, when making known the result of his calculation of the perturbations experienced by Halley's Comet from 1682 to 1759, that it might be subject to the influence of planets too distant to be perceived from the Earth, Le Verrier in conclusion expresses the hope that these distant planets may not be all invisible, and that it might be practicable to discover the body of which he had made known the position.

Le Verrier's third memoir, the most remarkable production of the three, was communicated to the Academy of Sciences on August 31, 1846. In the former investigation it had been assumed that the mean distance of the disturbing body was twice that of *Uranus*. Le Verrier now considered the mean distance one of the unknown elements to be determined, like the others, from the solution of his equations of condition; from which also the mass of the undiscovered planet would result. The necessary treatment of these equations was found to be very tedious and difficult, but finally the following definitive results were obtained:—

Semi-axis of the orbit	36·154
Duration of the sidereal revolution	217·387 years.
Eccentricity	0·10761
Longitude of perihelion	284° 45′
Mean longitude 1847, January 1	318° 47′

Mass $\frac{1}{9300}$ (the Sun's mass = 1).

From which elements Le Verrier found for the true longitude of the perturbing body on January 1, 1847, 326° 32′ and for its radius vector 33·06. He then proceeds to assign limits within which the elements given above might be varied without ceasing to represent the observations, and thus effectually to limit the space of the heavens within which it would be necessary to search for the planet so as to insure its true position being included. This space he found to be comprised between 321° and 335° of heliocentric longitude; but he considered that positions differing much from 326° 32′, the value directly found, possessed little probability, and suggested that the search for the planet should be commenced at the point immediately

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indicated by the solution of his equations. Further, with the mass he had derived for the disturbing body, and on the assumption that its density might be the same as that of *Uranus*, he considered the apparent diameter might be three seconds of arc, and therefore would be easily measurable in large telescopes. One of the most striking characteristics of this memorable paper is the confidence displayed by its author in the result of his investigations, or, as the Astronomer Royal has expressed it, "the calmness and clearness with which he limited the field of observation, and the firmness with which he proclaimed to observing astronomers, 'look in the place which I have indicated, and you will see the planet well.' . . . . It is here, if I mistake not, that we see a character far superior to that of the able, or enterprising, or industrious mathematician—it is here that we see the philosopher."

On September 18 Le Verrier addressed a letter to Dr. Galle, then in charge of the powerful refractor of the Royal Observatory at Berlin, begging him to make a search for the planet, of whose existence he felt so confident. The letter arrived on September 23, and the result is well known. Availing himself of Bremiker's Chart for Hour xxi. of R.A., one of the series issued by the Berlin Academy (which, though quite ready for publication, had not yet been circulated), he remarked, on the same evening, an object shining as a star of the eighth magnitude in a position where none was entered upon the map, the motion of which was strongly suspected on this night, and established beyond doubt on the following one, the change of place corresponding nearly to that which Le Verrier's elements of the new planet indicated. On the same night also a measurable disk was recognised, which the micrometer showed to have a diameter of between two and three seconds. The observed longitude was within 55′ of that which Le Verrier had predicted. It is unnecessary to expatiate here upon the admiration, and, as regards astronomers generally, the astonishment at the successful issue of the unprecedented labours of Le Verrier, which the announcement of this great discovery produced. It was only limited to the civilised world. Honours showered upon him from all quarters and his name may truly be said to have suddenly become "a household word" wherever astronomical science is cultivated.

The actual discovery of the planet was known in this country on September 30 by letters from the Berlin Observatory. Up to this time the entirely independent work of Professor Adams in the explanation of the irregularities in the observed motion of *Uranus* by the action of an undiscovered planet, and the determination of its actual position, was known only to a limited circle in this country, and, so far as appears, was not at all known abroad. Hence, when the official astronomers to whom Professor Adams' results, which were almost identical with those of Le Verrier, had been communicated, came forward immediately after the actual discovery of the planet at Berlin was announced,

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to state the circumstances under which his investigation had been conducted and had become known to them, animated and heated discussions arose, to which further allusion need not be made here, than to add that they soon subsided into a general feeling of respect and admiration for the two distinguished mathematicians who, unknown to each other, had successfully treated a problem which it had previously been considered almost impossible to solve.

In November 1846 Le Verrier published, as an Appendix to the *Connaissance des Temps* for 1849, *Recherches sur les mouvements de la Planète Herschel (dite Uranus)*, in which he gave a detailed account of the processes, analytical and numerical, involved in the course of his investigations, of which, previous to the telescopic discovery of the disturbing body, he had only given the particulars found in the three memoirs presented to the Academy and referred to above. It is to these *Recherches* that recourse must be had in order to follow Le Verrier through the various steps by which he accomplished the solution of the grand problem which added *Neptune* to the known great planets of the Solar System.

On the death of Arago, in 1853, Le Verrier was appointed to the direction of the Observatory at Paris, an institution which by his indomitable energy he raised from a position inferior to other principal national establishments, until he left it second to none. In 1870, in consequence of differences with the staff of the Observatory, he ceased his connection with it for a time, but on the melancholy death of Delaunay in 1872, he was reinstated, and retained the management of the institution during the rest of his life. It was while director of the Observatory of Paris that Le Verrier undertook and accomplished the immense work with which his name will be mainly associated in future times—no less than the complete revision of the planetary theories—the theories of *Mercury*, *Venus*, the Earth, *Mars*, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*, and the formation of tables of their motions. On two occasions these researches, which have been well termed appalling in their magnitude, have formed the subject of addresses from the Chair of the Royal Astronomical Society, first in the year 1868, when the Gold Medal of the Society was presented to Le Verrier at the hands of Professor Pritchard for his theories and tables of *Mercury*, *Venus*, the Earth, and *Mars*; and again in 1876, when, during the presidency of Professor Adams, the Society's Medal was for the second time adjudged to Le Verrier for the similar investigation of the theories of the four great planets, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*, and for his Tables of *Jupiter* and *Saturn* founded upon them. (The Tables of *Uranus* and *Neptune* were published at a later period.) The addresses delivered on these occasions, which, as usual, are printed in the *Monthly Notices*, enter into particulars of the nature and extent of Le Verrier's work in the planetary theories; Professor Adams' valuable analysis of the more intricate and laborious researches connected with the larger

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planets affording an admirable outline of Le Verrier's greater achievements. The circumstance that our late distinguished Associate's work upon the planetary theories has thus twice been the subject of detailed comment in the publications of the Society may render it unnecessary to enter into recapitulation here; but it may be useful for reference to subjoin the brief *résumé* as presented by Le Verrier to the Academy of Sciences at the beginning of the year 1875.

The general developments formed the object of five memoirs, presented and published in 1840, 1843, 1849, and 1855.

The formulæ relating to the secular inequalities were specially treated in the memoirs of 1840 and 1841. The same subject was taken up again, in a more general and complete form, in a work communicated to the Academy on November 11, 1872, concerning the four great planets, *Jupiter*, *Saturn*, *Uranus*, and *Neptune*.

The theory of *Mercury*, presented in 1843, afterwards completely revised, was definitively finished in 1859; the theory of *Venus* was given in 1861; that of the Sun (the Earth) in 1853 and 1858; that of *Mars* in 1861; the theories of *Jupiter* and *Saturn* in 1872 and 1873. The theory of *Uranus*, first communicated in 1846 on the occasion of the discovery of *Neptune*, was the subject of a new investigation, presented on November 15, 1874. Lastly, the theory of *Neptune* was presented to the Academy of Sciences in the first days of 1875.

These theories, and the tables constructed upon them, have been given to the world in the noble series of *Annales de l'Observatoire de Paris*, vols. iv. to xiv., which will remain a lasting monument to the great Astronomer.

One of the last subjects that engaged Le Verrier's attention was connected with the unexplained motion in the perihelion of the planet *Mercury*, to which he had pointedly referred upwards of twenty years previously, and which he considered to be due to the action of matter, in one form or another, between *Mercury* and the Sun; and on this point Professor Adams has remarked that "the theory of the planet has been established with so much care, and the transits of the planet across the Sun's disk furnish such accurate observations, as to leave no doubt of the reality of the phenomenon in question: and the only way of accounting for it appears to be to suppose, with M. Le Verrier, the existence of several minute planets, or of a certain quantity of diffused matter circulating about the Sun within the orbit of *Mercury*." Le Verrier was first led to suppose that the telescope might furnish proof of the existence of at least one such minute planet when he became acquainted with the observation of Lescarbault in March 1859. In 1876, on collecting five or six observations of a similar nature, he treated them with the view to discover whether they could be attributed to the transit of a single intra-Mercurial body across the Sun's disk, and, with a skill almost peculiar to himself, he found that they might be represented, so far as regards the times of conjunction with the Sun, by the motion of

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a single planet, the heliocentric longitude of which is given by a formula, with an indeterminate, admitting however of only two values without deviating considerably from observation. The period of revolution of such a planet will be either 27·96 days or 33·02 days, according as one or other of these values is adopted. Le Verrier considered that a transit of the hypothetical planet was possible in the spring of 1877, but that no other could occur before 1885. As is known, an energetic watch was maintained upon the Sun's disk at the critical period without success.

Le Verrier's health had long been in an indifferent state, and for some six months previous to his decease he had been unable to take part in the proceedings of the Academy of Sciences. Towards the close of his life he had been greatly assisted by M. Gaillot, when from painful and depressing illness he was prevented from giving active personal superintendence to the completion of his planetary tables. To the exertions of M. Gaillot it is believed to be owing that the last sheets of the Tables of *Neptune* were ready a few days prior to Le Verrier's death, and that thus he was enabled to see the greatest work of his life brought to a close. Le Verrier expressed in strong terms to the writer of this notice his feeling of appreciation of the value of the services which M. Gaillot had rendered him.

The death of M. Le Verrier occurred early on the morning of September 23, 1877. He was buried on the 25th, in the Cemetery of Mont Parnasse, at Paris. The funeral *cortège* left the Observatory under a military escort, with muffled drums—as usual at the funeral of a Grand Officer of the Legion of Honour—proceeding by way of the Avenue de l'Observatoire and Rue Val de Grace to the Church of Saint Jacques de Haut-pas, where the religious ceremony took place in the presence of a crowded assembly. The cordons of the pall were held by M. Peligot, President of the Academy of Sciences; M. Dumas, Vice-President of the Superior Council of Public Instruction; M. Fizeau, Vice-President of the Academy and Member of the Scientific Council of the Observatory; M. Faye, President of the *Bureau des Longitudes*; General Morin, Vice-President of the *Association Scientifique* (which active institution was founded by Le Verrier); General Baron Wrede, of the Academy of Stockholm; Captain Mouchez, of the *Institut*; and Mr. Hind, Superintendent of the *Nautical Almanac*. The Marshal-President of the French Republic was represented by one of his staff, the General-Commandant of Paris; and deputations from the École Polytechnique, at which Le Verrier completed his studies, and from other institutions with which he had been connected, were also present. Discourses were pronounced at the grave at Mont Parnasse by MM. Dumas, Faye, Janssen, Tresca, and Yvon Villarceau. These *éloges* will be found in the *Comptes Rendus*.

J. R. H.

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KARL LUDWIG VON LITTROW was the son of Professor J. J. von Littrow, formerly Director of the Imperial Observatory at Vienna and the author of several important papers on astronomical and physical subjects, some of which were contributed to the Royal Astronomical Society, and may be found in the first four volumes of the *Memoirs*. Before Dr. J. J. von Littrow's appointment in 1819 to the Observatory at Vienna, he held the Professorship of Astronomy in the University of Kasan, where his son Karl was born on July 18, 1811. The elder Littrow personally superintended the education of his son from an early age, and thus the latter soon acquired that taste for astronomical pursuits which ultimately led him to follow in the footsteps of his father as a professional astronomer. On the completion of his education, he accepted, in 1831, the position of assistant-astronomer in the Imperial Observatory of Vienna.

A few years after his appointment to the Observatory, Dr. K. von Littrow made a very valuable contribution to the history of astronomy by his publication of Father Hell's account of his observations of the transit of *Venus* in 1769 at Wardhoe, derived from that observer's original daybook, to which his official position gave him access. This work, *Reise nach Wardoe bei Lappland und seine Beobachtung des Venus-Durchganges in Jahre* 1769, was published in 1835, and it gives a faithful transcript of Hell's apparently doubtful observations, which had formed the subject of much unfavourable criticism since the publication of Encke's treatise on the transit of *Venus* in 1769. By a careful examination of a *facsimile* copy of the original, Mr. Stone, in his recent discussion of the observations of that transit, was able to explain much that appeared to be doubtful, and to interpret the entries in such a manner as to show the probable truthfulness of the observations. Soon after the appearance of this work, by which the name of the younger Littrow became known in our science, independently of that of his father, he made the first of his long series of communications to the *Astronomische Nachrichten* in the form of a note, containing the results of his observations of Halley's Comet, a class of work to which for some time he particularly devoted himself. It is not necessary here to give in detail the names of the various newly discovered planets and comets which occupied his attention, both before and after his father's death, but it will be sufficient to remark that in the *Astronomische Nachrichten* alone may be found abstracts of his equatoreal observations of more than thirty comets, in addition to similar abstracts of the results of his observations of most of the early-discovered minor planets, including several elements of their orbits. The results of his observations of the great comet of 1843, Colla's Comet, and others were communicated to this Society, and they appear in the *Monthly Notices*.

In 1840 the directorate of the Observatory of Vienna became vacant by the death of the elder Littrow, and there appears to have been some difficulty in choosing his successor. In 1842, however,

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the vacancy was filled by the appointment of Dr. K. von Littrow, whose qualifications for the office have been so well proved by his able administration during the last thirty-five years. He also succeeded his father in the Professorship of Astronomy in the University of Vienna. During his long superintendence of the Observatory the general operations of the establishment were confined chiefly to equatoreal astronomy, especially in relation to new cometary and planetary discoveries, in the carrying out of which the Vienna astronomers have always taken more than usual interest. Several important investigations on the orbits of particular comets have also been undertaken by Professor von Littrow and the assistant-astronomers, Hornstein, Oeltzen, and Weiss. The *Annalen der Sternwarte in Wien* contain the results of many of the most important researches emanating from the Observatory, among which may be found Oeltzen's Catalogue of Argelander's northern zone observations of stars between 45° and 80° declination reduced to 1842. This excellent catalogue appeared in two parts, one in 1851 and the second in 1852; and though it might with advantage be more conveniently arranged by the addition of columns with the annual precessions, so that the places of the stars might be more easily reduced to another epoch, still it is found of great use at the present time for the identification of many small stars observed with comets or other objects, but not included in other catalogues.

Professor von Littrow communicated several papers to the Vienna Akademie der Wissenschaften, which are printed in its *Sitzungsberichte*. He has also contributed to the proceedings of other societies. In the *Monthly Notices* there are several notes forwarded by him to the Royal Astronomical Society, including one on the rectification of equatoreals by observations of stars on the meridian and at an hour-angle of six hours; and another on a method of determining the latitude at sea. In 1864 he requested M. Faye to lay before the French Academy of Sciences a new method for determining the time and the longitude at sea, which was tried during the scientific voyage of the Austrian frigate, the *Novara*, and adopted by Admiral von Wüllerstorf as part of the daily routine of the vessel. This method consists in the determination of the time by two circum-meridional observations of the Sun, preserving at the same time the observation of the true noon for latitude, the two altitudes being taken at pleasure on the same side or on the opposite side of the meridian. Professor von Littrow was also the editor of his father's well-known treatise on popular astronomy, *Die Wunder des Himmels*, which has passed through several revised editions. This work is divided into four parts, the first of which treats of theoretical astronomy; the second part relates chiefly to descriptive astronomy and the topography of the heavens; the third is devoted to physical astronomy; while the fourth refers to the explanation of the construction and use of astronomical instruments. The atlas, which forms a suitable addition

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to the work, *Atlas des gestirnten Himmels für Freunde der Astronomie*, has also been revised by Dr. K. von Littrow.

It was principally through the representations of Professor von Littrow, that the Imperial Academy of Sciences at Vienna proposed in 1869 a scheme for awarding a series of prizes, limited to eight annually, for the discovery of telescopic comets during the three years' interval between May 31, 1869, and May 31, 1872. The prizes to be awarded consisted either of a sum of money—twenty Austrian ducats—or of a gold medal of equivalent value. The scheme was faithfully carried out, and before Professor von Littrow's death it was proposed to the Academy to continue the prizes for another term.

The situation of the Vienna Observatory, in the midst of narrow streets, has long been felt as a serious hindrance to the prosecution of any delicate astronomical work. The present building was erected in 1826–27, under the direction of Professor J. J. von Littrow, on the site of the old Observatory, founded in the year 1753, Father Hell being the first director. The requirements of astronomy of the present day in a national Observatory induced Professor von Littrow to represent to the Austrian Government the desirability of removing the Observatory to a more favourable position out of the city. This recommendation was acceded to, and in 1873 the sanction of the Minister of Public Instruction was obtained for the erection of a building capable of containing instruments of the largest construction, and fitted with every astronomical appliance required in the pursuit of meridional, equatoreal, or physical observations. The foundation stone of the new Observatory was laid in June 1874, about three miles to the north of the centre of Vienna, and at the present time is approaching completion. Professor von Littrow was not permitted to see the full effects of his design, but the works were sufficiently advanced before his death to enable him to foresee the material advantage which astronomy has gained by the increased accommodation in the new Observatory, and the absence of the noise and dirt which so injuriously affect the observations in the present building. Among the instruments which it is proposed to erect, the great refractor of 26 inches aperture is the most noted. This instrument is in progress of construction by Mr. Howard Grubb, of Dublin.

Professor von Littrow was elected an Associate of our Society in April 1853. He was also a corresponding member of several other scientific societies. His death took place on the morning of November 16, 1877, at Venice, where he had gone for the benefit of his health. He was in the sixty-seventh year of his age. E. D.

Giovanni Santini was born on the 30th of January 1786 or 1787 (both dates have been mentioned), at Caprese, near Borgo di San Sepolcro, in the province of Arezzo, Tuscany. In early

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life he studied astronomy with Piazzini at Pisa, and afterwards he placed himself under the direction of Oriani, director of the Observatory in the Jesuit College at Milan, and of his assistant, Cesaris. In November 1806 he was appointed an assistant astronomer in the Observatory at Padua, where in a few years he became known as one of the rising young astronomers of the age. So early as 1808 and 1809 we find that Santini was a contributor to the *Monatliche Correspondenz* of Zach, and his papers in that publication, containing observations of a recently discovered comet, and of the minor planets *Ceres* and *Vesta*, gave indications of his future successful career as a practical astronomer.

In 1813 Santini received the appointment of Professor of Astronomy in the University of Padua, and also of Director of the Observatory. While strictly devoting himself to the duties of his professorship, he never neglected those of the Observatory, and he lost no opportunity of observing any newly discovered comet, and the first four minor planets, especially of *Vesta*, which he continued to observe regularly at every opposition for more than twenty years after its discovery. During a large portion of his long career, miscellaneous observations of this kind continued to be his favourite occupation, and an examination of the titles of his papers shows that the greater number of his communications to Societies, the *Astronomische Nachrichten*, and other serial publications relate chiefly to this branch of equatoreal astronomy.

In 1837 Professor Santini resolved to inaugurate a new system of observations at the Observatory of Padua. In that year an excellent meridian-circle was erected, and with this instrument he was induced to undertake some special meridional work in addition to that which had hitherto occupied his time; but in doing this he determined still to continue occasionally his equatoreal observations. In his ordinary measurements of differences of right ascension and declination between comets or minor planets and neighbouring stars, Santini often experienced much inconvenience from the difficulty of finding in existing star-catalogues a sufficient number of stars suitable for comparison whose places had been satisfactorily determined. At that time the reduced places of the zone-stars observed by Bessel were not available, nor was there any other catalogue of small stars which could give much assistance to the observer. He therefore considered that his new instrument could not be employed in more useful work than in supplying well-determined places of a selected number of small stars suitable for the required purpose. He therefore planned a scheme of zone-observations of stars in right ascension and declination with the new meridian-circle, so that in each parallel of declination, within certain limits north and south of the Equator, there should be found a star, at every eight or ten minutes of time, whose place could be depended upon. Santini commenced his observations early in 1838, and all the observations made from that date to

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the autumn of 1840 were reduced and incorporated into a Catalogue of 1677 stars. He communicated this catalogue, the first of a series, to the Royal Astronomical Society in December 1840, under the title of a *Catalogue of* 1677 *Stars included between the Equator and* 10° *of North Declination, observed at the Royal Observatory of Padua.* It is inserted in vol. xii. of the *Memoirs.* The places derived from this catalogue were at once recognised as being well determined, and they have been found of great service in comparison-observations of the minor planets and comets. This practical confirmation of the desirability of an extended system of observation of the positions of small stars, at a time when there was no reduced catalogue of these objects in existence, had some influence in encouraging Santini to continue his observations of the remaining zones which formed part of his original scheme. These zone-observations were therefore continued at Padua under Santini's direction, either by himself or by his assistants, Trettenero, who died in 1863, and Lorenzoni. Several instalments have appeared from time to time, each containing the reduced places of a large number of stars, and the complete series of catalogues forms a very valuable contribution to the libraries of astronomers. Excepting the first catalogue, they were communicated to the Istituto Veneto di Scienze, and published in the *Memoirs* of that Society. Our own *Memoirs* and *Monthly Notices* contain many records of Santini's labours, chiefly observations of comets in various years, and of the planets *Ceres*, *Pallas*, and *Vesta* in 1826; observations of the right ascension of *Venus* about her inferior conjunction in 1830, and miscellaneous notes. In an important paper on the solar eclipse of July 1851, Professor Santini has discussed the different observations made in various parts of Europe, from which he has deduced corrections to the lunar tables of Burckhardt in longitude and latitude, and to the sum of the semidiameters of the Sun and Moon. By substituting the resulting values of the three corrections in the equations of condition, he found that they satisfactorily represent the residual errors. His papers on the variations in the elliptic elements of the periodical comet of Biela, produced by the action of *Jupiter*, *Saturn*, and the Earth, deserve also a special mention.

Few astronomers have had the opportunity of devoting so many years to the service of astronomy as Santini. The best proof of the activity of his life as an observer is contained in the *Catalogue of Scientific Papers* prepared by the Royal Society. Though the first section of this work only includes papers published before the end of 1863, we find in it the titles of 137 separate astronomical papers duly recorded. He was also the author of an excellent treatise on *The Elements of Astronomy*, and of another on *The Theory of Optical Instruments.*

At one period of his life the health of Professor Santini gave great anxiety to his friends, which he himself evidently shared. In 1840, having scarcely completed his first series of zone-

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observations, he, at the age of fifty-four, wrote of the rapidly declining state of his health, and he expressed doubts whether, at his age, with weak health, he would be able to accomplish the work which he had laid down for himself to do. He knew, however, that his scheme of zone-work, if successfully carried out, must prove useful to astronomers, and that, if his illness continued, others would probably undertake to complete the observations of the remaining zones. His illness and its accompanying anxieties fortunately passed away, and with the valuable assistance in the Observatory of Signors Trettenero and Lorenzoni, he lived to see the general completion of his original plan of observations and the publication of the reduced mean places of the stars for the respective epochs of the catalogues.

In early life Santini entered the Church, in which he had priest's orders. This, however, never interfered with his professional work as the Professor of Astronomy in the University, or as the Director of the Observatory. In private life he was universally beloved and respected, and though he survived to the great age of ninety-one, his death caused a blank among his scientific friends which will be difficult to fill. He died on the morning of the 26th of June 1877, at his residence at Padua.

Santini was elected an Associate of our Society so long ago as March 1825. E. D.

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Proceedings of Observatories.

The following Reports of the proceedings of Observatories during the past year have been received by the Council from the Directors of the several Observatories :—

Royal Observatory, Greenwich.

The Transit Circle and Altazimuth have been used regularly for the ordinary observations of the Sun, Moon, planets, and fundamental stars, on the same system as in past years. The only change to be noticed is the observation of stars by reflexion in R.A. with the Transit Circle as a check on the validity for different zenith distances of the adopted level-errors. Two clock stars, as well as close circumpolar stars, have been observed by reflexion in R.A., as far as practicable, on every observing night throughout the past year. Upwards of 70 stars have been observed, on an average four times each in R.A. and N.P.D., at the request of Mr. Gill for comparison with *Mars*, *Ariadne*, and *Melpomene*. For the coming year a new working catalogue has been prepared, which includes all stars down to the fifth magnitude not observed in the last catalogue, a list of 260 stars for the United States coast survey, and other stars required for various purposes, the whole number amounting to 2,600. The azimuth-error of the Transit Circle has been steadily increasing for some years past, and it has been found necessary to reduce it by shifting one of the Y's.

With the Great Equatoreal a number of drawings of *Mars* have been made, forming a record of a complete rotation of the planet, and estimations of the positions of the two satellites have been obtained. A few measures have also been made of some of the satellites of *Saturn*, including *Enceladus*. Occultations of stars by the Moon and phenomena of *Jupiter's* satellites have also been observed at every available opportunity.

The spectroscopic work has been somewhat interrupted by the illness of Mr. Maunder, as well as by these observations, and by the various small alterations consequent on the substitution of the new Half-prism Spectroscope for that formerly used. The chromosphere was examined with the Ten-prism Spectroscope (in use for solar work till the end of July) on 21 days in the seven months from January 1 to July 31, and with the Half-prism Spectroscope on 56 days in the following six months to 1878, February 1. On 20 days out of the entire 75 no prominences were seen. The Half-prism Spectroscope has also been used for the examination of the spectrum of the Sun-spot of October 25–November 6, and in the search for bright lines in different parts of the solar spectrum. The old spectroscope was

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in use for stellar work up to May 2, the displacement of 17 stars having been measured with it, and those of 15 with the Half-prism Spectroscope, which has been in use for this work, with some interruption, from August 23. The Single-prism Spectroscope has been used in the examination of the spectrum of the Moon during the total eclipse of August 23, of *Mars*, and of some variable stars.

Observations of the "rainband" in the sky spectrum, to which attention has been called by Professor Piazzi Smyth, have been recently commenced and are now made regularly with a small Half-prism Spectroscope.

The lines in the spectrum which occasionally intrude into that of hydrogen in some vacuum tubes have been measured, and the results show, as Dr. Schuster has pointed out, that the lines may presumably be referred to the vapour of zinc, which is used in the preparation of the hydrogen.

During the year 1877 photographs of the Sun have been taken on 169 days; in most cases small groups of faculæ are shown, but on 49 days no spots are visible. The areas, position-angles, and distances of spots and faculæ from the Sun's centre have been completely measured in duplicate up to the end of 1876.

The computations for the *Nine-Year Catalogue of Stars*, which are now complete, have pressed heavily on the computing staff during the past year. All the MS. is now ready for the printer, and the Catalogue has been printed as far as 5^h. The total number of stars is 2,263. All the N.P.D.'s are corrected for error in the assumed colatitude determined from all observations of circumpolar stars in the Catalogue. The new value of colatitude, based on 5,976 observations above Pole and 2,685 below, is 38° 31′ 21″·40, being 0″·20 less than the value adopted in the annual volumes since 1867. For the determination of the systematic errors in Right Ascension, a comparison has been made of the R.A. of clock stars found from groups which extend over 12^h at least and those of the Catalogue, and also a comparison of the R.A. of wide circumpolar stars above and below the Pole. The corrections to the R.A. of the *Nine-Year Catalogue*, as deduced from 12-hour groups, range from $+0^s{\cdot}013$ about 0^h to $-0^s{\cdot}010$ about 9^h, as depending on the R.A., and from $-0^s{\cdot}005$ at 50° to $+0^s{\cdot}003$ at 100° and upwards as depending on the N.P.D. These corrections have been applied in forming the Assumed Mean R.A. of clock stars for 1878 from the *Nine-Year Catalogue*. From all the observations of circumpolar stars the mean excess of R.A. above Pole over that below corresponds to $+0^s{\cdot}009$ in equatoreal interval. The observations have been tabulated in order of R.A. as well as of N.P.D., but the discussion is not yet complete.

Notwithstanding this extra work, the ordinary reductions are in a forward state, the Mean R.A. and N.P.D. Jan. 1 being

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formed for all observations to the end of last month (1878, Jan. 31). The ledger of stars' N.P.D. for 1877 is prepared.

The volume of *Observations for* 1875 has been distributed, and the printing of that for 1876 is far advanced.

Radcliffe Observatory, Oxford.

The observations during the past year, excepting those relating to the planets *Mars* and *Saturn* at opposition, having been of the usual character, a few words only are necessary with regard to them.

The number of stars observed has been about the average, and the amount of planetary observations also. It may, however, be mentioned, that 28 meridional observations of *Mercury* were made in the year. The observations of solar spots have been continued without interruption, as in former years, and a considerable number of observations of the phenomena of *Jupiter's* satellites and 7 of occultations of ordinary stars by the Moon (in addition to 10 of very small stars made during the total lunar eclipse of August 23, 1877) have been made. Of Mr. Gill's list of stars to be observed in comparison with *Ariadne*, 12 have been observed on the meridian, many of them several times.

During the autumn considerable attention was, of course, paid to the planet *Mars*.

On the meridian he was observed, together with a considerable number of the stars to be compared with him (especially those in Mr. Gill's list, of which 27 have been observed), forty-nine times.

With the Heliometer the observations have been in general confined to measures of the disk (of which 13 were made), though some measures of his distance from comparison stars have been made also. A great deal of pains has also been devoted to *Saturn*, the ring being so thin as to offer no impediment to the measures necessary for again determining his ellipticity. The number of measures of the equatoreal and polar diameters which has been made is 25. At the time of the opposition of *Mars* it may be mentioned that the weather was not very favourable for observations.

With regard to the reductions, it may be stated that some advance has probably been made on last year. All astronomical results of every kind for 1876 are complete, but the meteorological reductions are still considerably in arrear. Of that year the results for R.A. are in the hands of the printers, and one sheet has been printed, the results for N.P.D. being also ready. For the year 1877 the reductions of the transits are completed, and for the zenith-distance observations the zenith points have been computed for a considerable portion of the year.

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A considerable advance has been made in the reductions connected with the Great Catalogue of Stars. The mean results, as derived from the different years of observation (reduced to 1866), are nearly complete, and will soon be written into the formal Catalogue, and the bulk of the remaining work will be the computation of the precessions and secular variations.

With regard to the printing, it may be mentioned that the volume for 1874 was distributed shortly after the annual meeting of the Society in February 1877, and that the following volume for 1875 is completely printed and will be issued very shortly.

This volume, in addition to the usual matters, contains for the first time the observations and delineations of the solar spots made with the solar eye-piece attached to the Heliometer. The drawings made by Mr. F. Bellamy have been engraved, and many very interesting peculiarities of the spots have been put in evidence. It is intended to continue the publication of these observations from year to year, and it is hoped that, from the accuracy of the observations of the positions in R.A. and N.P.D., and the pains which have been taken with the drawings, the series will prove to be of considerable value.

Some delay was occasioned in the printing of the volume by the engraving of the drawings; otherwise it would have been published considerably earlier.

It has been already mentioned that the printing of the volume for 1876 is begun.

Before concluding, it should be stated that a severe watch was kept up for the planet *Vulcan*, from sunrise to sunset, at each of the epochs denoted by M. Le Verrier as giving a probability of a passage across the disk of the Sun.

Oxford University Observatory.

During the last twelve months the usual operations in this Observatory have been steadily pursued. A considerable series of measures of the Satellites of *Saturn* has been completed. Five comets of the year 1877 have been systematically observed, and ephemerides have been computed and published from elements obtained from Oxford observations.

Two hundred double stars have been measured, and orbits have been calculated for 70 *Ophiuchi*, ξ *Ursæ Majoris*, and μ *Boötis*, by a process which seems to be a practically useful modification of the graphical method, combined with another proposed by Savary. The resulting elements represent observations somewhat more closely than has hitherto generally been the case. The experiences gained by the construction of these orbits, together with the loss of time and annoyance caused by the discrepancies of almost all the older observations, and in very many of the more modern,

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indicate the desirability of in future confining the attention to a very small list of stars for this Observatory, with multiplied observations thereof, and the exercise of the greatest caution. The maxim "non multa, sed multum" seems here to apply with peculiar force.

A few observations of the outer satellite of *Mars* have been made, and one, it is believed, of the inner satellite. Upwards of five hundred measurable photographs of the Moon have been secured, and it is hoped that they may be submitted soon to the scrutiny of Mr. De La Rue's superb measuring instrument now just completed by Mr. Simms.

It is obvious that the above results could not have been brought about without the zealous and skilful co-operation of the two assistants, Mr. W. Plummer and Mr. Jenkins. Valuable aid has also been constantly afforded by our skilled mechanic Mr. Mullis.

The delegates of the Clarendon Press have agreed to print and publish the results of the foregoing observations, together with those made in 1876, and the series of observations of *Saturn's* Satellites made in the autumn of 1875; the instruments having been fixed in their places only the summer of the latter year. It is expected that a small volume containing the above will be submitted to the Society at their Monthly Meeting in March next.

A new form of Micrometer has been successfully contrived and completed for the large Grubb Refractor, with the view of obtaining differences of right ascension, differences of declination, and distances with the utmost accuracy, and extending to *much wider intervals* than has heretofore been generally practicable with the ordinary filar micrometer. The adaptation of this somewhat powerful instrument to the large Refractor rendered it essential to ascertain the amount of the exactness and uniformity of the central telescopic field; and the satisfactory result has been that, for a central circular field of at least 20′ diameter, there is no distortion amounting to 0″·1. This then, at least, may be considered as the optical and measuring capacity of the 12·4 inch Refracting Telescope with its new micrometer, viz.:—That it admits of measuring intervals of at least 20′ in any direction, with an accuracy of 0″·1, and that with any magnifying power that the telescope can bear at the time. A description of the instrument will shortly be submitted to the judgment of the Society.

The University have added a Lecture Room and a Library to the Observatory Buildings, for the instruction of academical students in Astronomy. The dimensions of the former are 40 feet long, 25 feet broad, and 18 feet high. The architect, Mr. Charles Barry, has promised it with a flat lead roof and parapet. Over the roof, but not touching it, are certain firm and strong platforms, intended to carry a few instruments, such as a reflector of 10·4-inch aperture, a refractor 4·4-inch aperture, an altazimuth with 15-inch circles, a sextant, and a transparent star

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drum of 3 feet diameter, on which are projected the principal stars and celestial circles; the cylinder turns on an axis parallel to that of the Earth, and is provided with a plane horizon. Such an out-of-door observatory, or open-air lecture room, seems to promise certain advantages for academical instruction not possessed by a closed observatory.

Oxford, January 18, 1878.

Cambridge Observatory.

The work of the Observatory has still been chiefly devoted to observations of the zone stars and to the reduction of those observations. To this end 4,703 observations of stars in Right Ascension and North Polar Distance have been made during the year, including 3,745 observations of zone stars, 744 of clock stars, and 214 of slow stars for the determination of instrumental deviation; 157 of these last observations being direct, and 57 by reflection. The zenith point was determined, with hardly an exception, on every night on which observations were made, and this operation is now facilitated by means of stages, which are easily movable and which have been specially constructed for observing by reflection. The level and collimation errors have also been regularly determined. The reductions are in an advanced state.

As one of the wires had to be replaced toward the end of last year, a redetermination of the intervals became necessary. These have been obtained from all the suitable observations, both of clock stars and of *Polaris*, which have been made throughout the year, and the requisite tables are in process of construction.

An elaborate series of observations were made in August last to test the form of the transit pivots. These observations have led to the satisfactory conclusion that the corrections due to the form of the pivots are so small that they are scarcely worth taking into account.

They are well represented, in the eight symmetrical positions of the instrument in which the observations were made, by the formulæ:

$$e = {}^s\cdot010 \sin(207^\circ\ 50' + \Delta) + {}^s\cdot010 \sin(66^\circ\ 43' + 2\Delta) + {}^s\cdot008 \sin(328^\circ\ 29' + 3\Delta)$$

for direct observations,

and

$$e = {}^s\cdot010 \sin(76^\circ\ 36' + \Delta) + {}^s\cdot010 \sin(322^\circ\ 9' + 2\Delta) + {}^s\cdot008 \sin(164^\circ\ 48' + 3\Delta)$$

for reflected observations;

where e is the deviation from the meridian, expressed in so-called seconds of time, one such second being equal to 15 seconds of arc,

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and Δ is the North Polar Distance. The term in 4Δ was inappreciable.

This shows that, if there is any wear of the pivots, it has been remarkably uniform, and therefore that Mr. Simms was highly successful in his endeavours to secure perfect homogeneity in the steel of which the pivots are made.

In compliance with a request from M. Le Verrier, a search for *Vulcan* was made in March last with the Northumberland Equatoreal, but without result. We have not been able to see the satellites of *Mars*, although they were looked for on several occasions.

Dunsink Observatory.

The Meridian Circle has been employed since the date of the last report in following up the scheme of observation therein referred to. Upwards of 400 observations of *Polaris* in zenith distance, at its upper and lower culminations, have been made. The observations of the red stars of Schjellerup's Catalogue are also nearly concluded.

Attention has also been paid, when practicable, to the stars which Mr. Gill proposed to observe with *Ariadne*, *Mars*, and *Melpomene*, the approximate places of which were published in the *Ast. Nach.*, No. 2141. The majority of these objects have been observed four times in R.A. and Decl., i.e. twice in each of the principal positions of the instrument, and the reductions have made some progress.

The principal work of reduction now in hand is the determination of the places of the minor planets observed at Dunsink since 1873, and the comparison of these observed places with the published Ephemerides, chiefly those of the *Berliner Jahrbuch*.

The transmission of Local Mean Time to the office of the Dublin Port and Docks Board has been continued during the year without interruption, except from the effect of a storm which carried away the transmitting wire and interrupted communication for some hours.

The South Equatoreal has been employed almost exclusively in observations for Annual Parallax of certain stars.

Royal Observatory, Edinburgh.

The work performed at the Royal Observatory, Edinburgh, during the past year has been :—

The service of astronomically procuring and daily distributing correct time by means of electric time-ball, time-gun, and controlled clocks, and keeping accounts thereof.

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The computations of the Meteorological Observations taken twice a day throughout the year, at 55 stations of the Scottish Meteorological Society, and furnishing the digested returns to the Registrar-General in Scotland, for his printed reports every month and every quarter.

The careful passing through the press of volume xiv. of the *Edinburgh Astronomical Observations*, containing about 700 pages and 22 plates.

Of the above pages, 640 go towards a first instalment of the "Edinburgh Star-Place Catalogue and Ephemeris," 26 are connected with the Earth-Thermometers, 21 with Spectroscopy, and 10 with successive Reports, addressed to the Board of Visitors since the appearance of the last volume in 1872.

Glasgow Observatory.

The operations at the Glasgow Observatory during the past year have been mainly confined to the reduction of the meridional observations of stars which had accumulated from the labours of former years. A few observations of stars for comparison with *Melpomene* were made at the request of Mr. Gill. The weather was unfavourable for the observation of *Mars* at the time of opposition. The system of time signals now in connection with the Observatory has recently engaged the attention of the Town Council of Glasgow, and will probably ere long receive a considerable extension.

Kew Observatory.

The astronomical work of this Observatory has been confined during the past year to the continued measurements of the Kew Sun-pictures taken during the years 1862–71, which has been carried on at Mr. De la Rue's expense and under his direction.

On account of the large number of spots which appeared on the Sun's disk during the year 1870, it has not yet been found possible to complete that year's work.

The largest number of spots found to exist on a solar photograph up to the present time is 152. These were registered on September 22, 1870.

The reduction of the measurements to heliocentric elements has been continued by Mr. Marth for Mr. De la Rue.

The eye observations of the Sun after the method of Heinrich Schwabe have been made daily, when possible, in order for the

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present to maintain the continuity of the Kew record of Sun-spots.

The magnetical and meteorological work, to which the attention of the Observatory is chiefly directed, has been prosecuted continuously, and the verification department has been fully occupied the whole year.

Liverpool Observatory, Bidston, Birkenhead.

During the past year differences of Right Ascension and Declination have been obtained, with the Equatoreal of this Observatory, of the comparison stars for the opposition of *Mars*—*Ariadne* and *Melpomene*. For giving time to the Port and testing chronometers, transit observations have been regularly taken, and the time has been carried on between one series of observations and the next following by means of three clocks, which have been compared daily with each other by coincident beats. In testing chronometers at this Observatory during the years 1873 to 1876 inclusive, the practice has been to furnish mariners with the daily rates in three different temperatures, and to supply them with the means of calculating the rates for other temperatures to which their ships might be exposed at sea. During the past year these calculations have very frequently been made at this Observatory, and the rates supplied for every five degrees from 45° to 95° of Fahrenheit. When this is done it is found that merchant captains have no difficulty in using rates corrected for change of temperature.

The Pacific Steam Navigation Company have provided for ships printed forms adapted for this method of applying a variable rate in accordance with the change of temperature at sea, and the results at present obtained are very satisfactory. Each of these ships carries three chronometers, *a*, *b*, *c*. The forms are so arranged as to show the daily error of each chronometer in Greenwich mean time, as obtained by the application of rates corrected for change of temperature. The differences between *a* and *b* and *a* and *c* are obtained by comparison of the chronometers with each other, and the last column in the form shows the daily differences between calculation and comparison for each pair of chronometers throughout the voyage. These records will in a short time furnish most valuable information with regard to the performance of chronometers at sea.

Temple Observatory, Rugby.

The greater part of the year just elapsed has been occupied in moving from the old Observatory to the new building, and in

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thoroughly cleaning the instruments. The principal work done, as heretofore, has been the measurement of double stars. 67 complete measures have been made, many of which are repetitions on the same star. Mr. Wilson has, with Mr. Gledhill, prepared and published, in the *Monthly Notices* of December 1876, a list of 422 binaries and interesting doubles, measures of which at intervals are desirable.

In the *Memoirs* of the Society, vol. xliii., will be found a Catalogue of 398 double stars measured during the last three years by Messrs. Wilson and Seabroke. The spectroscope on the Reflector has been used by Mr. Seabroke for the purpose of measuring the velocity of stars in the line of sight; the results, however, are not sufficiently reliable for publication.

The new Observatory was built during the summer months on a piece of land purchased by the Governing Body of the school for this purpose. It consists of a circular room 16 feet in diameter, with a conical roof, containing the Equatoreal; a computing room 16 feet square on the south of it, and a transit room 14 feet by 7 on the west.

An eight-roomed house in which the sub-curator resides is built at the entrance to the ground. A curator and sub-curator have been appointed to take charge of the Observatory.

The Observatory is open for members of the school during specified hours on each day, and thus the experiment is being tried of endeavouring to interest boys in observation, and to instruct them in observing and in the ordinary work of the Observatory. Some boys thus attracted to the science may hereafter join that band of amateurs to whom is owing much of what is most characteristic of English Astronomy.

Stonyhurst Observatory.

The routine work of this Observatory has been carried on without interruption during the past year. The daily work includes a complete photographic registration of magnetic and meteorological changes; monthly determinations of the absolute values of the elements of terrestrial magnetism are never neglected; and *Jupiter's* satellites are observed, and measures of double stars taken, as often as possible. Clear nights happily favoured the observation of the total eclipse of the Moon and the search for *Vulcan*, but not for the November meteors.

The object-glass of the large Equatoreal was always considered very inferior when compared with its splendid mounting, the inferiority being due to a large amount of spherical aberration. The glass has been examined by the optician who made it, and he finds the material excellent, and is confident that the convex lenses can easily be repolished so as to correct perfectly the aberration. The instrument is at present in his hands, but will be remounted as soon as possible.

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Mr. Barclay's Observatory.

The past year has been devoted to the continuation of double star observations, and the observations of comets, eclipses, occultations, and phenomena of *Jupiter's* satellites. The fourth volume of *Leyton Observations* is in the hands of the printer and will soon be ready for distribution. This volume will include December 31, 1877.

Colonel Cooper's Observatory, Markree.

The sky in this part of Ireland is too seldom and too irregularly clear to allow of the observation of variable stars with any chance of success; but the observation of binary stars has been continued through the year. As well as the ordinary meteorological observations, Dr. Doberck has spent much of his time in getting the old instruments into order. The Observatory is so remote from the aid of opticians that much has to be done by the observer himself. Considerable additions have thus been made to a small chronograph which has recently been acquired, and which now works satisfactorily.

Mr. Edward Crossley's Observatory, Bermerside, Halifax.

In the year 1877 not much observing work was done at this Observatory. The bad weather, an inefficient driving clock, and a faulty micrometer were the principal hindrances. In the early months of the year the usual double star measures were made, and some of the phenomena of *Jupiter's* and *Saturn's* satellites have also been observed. The greater portion of the year's work was done in the computing room, and it consisted in the reduction of the positions of double stars, the collection of measures, &c. Now that a new driving clock by Mr. Grubb has been fixed, and that a good micrometer by Messrs. Troughton & Simms is expected shortly, it is hoped that the observing will soon receive its usual share of attention.

Mr. Huggins' Observatory.

During the past year, owing to the unfavourable weather for observing which has prevailed over considerable periods, there has not been much opportunity for continuing work such as the

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photography of the spectra of the stars and planets, which is so delicate in its nature as to require atmospheric conditions exceptionally good. The few sufficiently clear nights have, however, been used; and further photographic spectra of *Sirius*, of the planets *Mars*, *Venus*, *Jupiter*, and *Saturn*, and of the Moon, have been obtained.

Lord Lindsay's Observatory, Dun Echt, Aberdeen.

During the year 1877 considerable progress has been made in the reduction of the observations taken at Mauritius. The deduction of the Solar Parallax, by Mr. Gill in London, from his heliometric observations of *Juno*, has already been published in *Dun Echt Observatory Publications*, vol. ii., to which may be added the final reduction of the time observations and the subsequent determination of the longitude of Mauritius from transport of chronometers, and also from Moon culminations and occultations.

Schmidt's *Nova Cygni* was observed on 12 nights at the beginning of the year, and again on 21 nights in the autumn, after the detection of the remarkable simplification of its spectrum on September 2. The position of the *Nova* with respect to above 50 closely adjacent stars has been micrometrically determined. This work has cost much time, but the desirability of possessing an accurate map of the stars in the neighbourhood in the event of its complete disappearance is sufficiently obvious.

Of the various comets which have appeared during the year, *a* was observed on 6 nights, *b* on 11, *c* on 4, *e* on 2, and finally *f* was viewed on one occasion with ordinary eye-pieces and with the spectroscope, although intervening clouds prevented complete observations.

Ten occultations of stars were secured, occasionally two, or even three, telescopes being used. On March 23 the remarkable 'flats' so long known to exist on the Moon's preceding limb at particular phases of libration were referred to the exact parts of the Moon's surface by which they are caused, a result fully confirmed by an observation of the terminator on July 25. The eclipse of the Moon on February 27 and August 23 were both observed, the latter to some extent photometrically.

The Great Refractor was tried also, as opportunity offered, on various close double stars, e.g. *ω Leoni*, *η Coronæ*, and γ^2 *Andromedæ*, which were all readily measured in distance and position. On the contrary, the satellites of *Mars* were never satisfactorily made out, probably owing to the low altitude of the planet; the latitude of Dun Echt, 57° 9′·6, necessarily prejudicially affecting the observation of objects far south of the Equator. Out of 96 nights indicated for comparison of *Mars* in declination with

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neighbouring stars, 28 only were at all clear, and serviceable observations could not be secured on more than 18 of these. The remainder of the year was, however, by no means so unfavourable to astronomical work.

Thanks to Mr. Marth's predictions, the shadow of *Titan* was observed in transit on November 23, and again on December 25.

The more interesting observations have been published in the *Monthly Notices* or the *Astronomische Nachrichten*. The whole, with full particulars, will be given in a future volume of the *Dun Echt Publications*.

The time-gun was fired on every week-day (with few exceptions) at Greenwich mean noon; the misfires have been mostly traced to damp fuzes, and can now probably be almost entirely prevented.

The Earl of Rosse's Observatory, Birr Castle.

During the year 1877, 143 sets for objects were made with the 6-foot Reflector in the course of the 39 nights on which observations were made. Our first object during the past, as in the previous year, has been the clearing up of doubtful points which had arisen in preparing the observations of nebulæ from the date of the erection of the 6-foot instrument up to the present time, for full and complete publication. Those for the first eight hours of Right Ascension are now almost ready for the press.

Mars was examined on 20 nights, chiefly with the 3-foot, and 15 drawings made on the most favourable occasions. The outer satellite was seen twice with the 6-foot, and the search was then discontinued, as the glare of the planet was found too strong to allow of good measures being taken.

Venus was examined on several occasions and some irregularities in the shape of the horns noted.

The radiant heat of the Moon was measured as far as the weather permitted during the eclipse of Aug. 23, also on several other nights for the determination of the value of the scale readings.

On re-examination of *Merope* with a finder eye-piece of low power and large field (power 130, field 26′)—of late years disused, nebulosity of a cometary form, extending on one side from the star, was distinctly perceived, though with higher powers it still remains unnoticed.

Some trials recently made with a small spectroscope tend towards confirming our opinion that in ordinary states of the atmosphere, and at any rate when a slit is not employed, the additional brightness of the spectrum with a speculum of large aperture does not compensate for the inferiority of the definition. However, we intend to make a fuller trial.

Jan. 12, 1878.

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Colonel Tomline's Observatory.

During the past year observations have been made of all the comets that have been discovered in that period, with the exception of D'Arrest's periodical comet only. These have been wholly or partially reduced, and it is intended to publish the results as soon as accurate places of the stars of comparison can be obtained. The want of astronomical literature, and particularly of star catalogues, is found to be a great drawback to the successful prosecution of the work which is regarded as the proper sphere of labour for this Observatory.

The reduction of the transit observations made in 1875 and 1876 has been nearly completed, and has led to some curious results, which will be communicated to the Society in detail very shortly. It would appear that the azimuthal and level errors of the Transit Instrument are subject to an annual fluctuation which is extremely regular in its development and is believed to arise in consequence of the radiation of heat from the adjacent and very massive pier of masonry which serves to support the Equatoreal Telescope.

Some attempts to determine the amount of light received from the totally eclipsed Moon on August 23, though only partially successful, showed that only a very small fractional portion of the Sun's light was refracted into the shadow cone by the Earth's atmosphere; but it has been found necessary to postpone entirely for the present the project to determine the photometric value of the magnitudes of the fainter stars.

Royal Observatory, Cape of Good Hope.

The positions of more than three thousand stars have been fixed with the Transit Circle of the Cape Observatory during the year 1877. These stars have consisted chiefly of Lacaille stars which at the present time fall within the zone from 125° to 135° N.P.D.; but many southern stars from Brisbane's Catalogue have also been observed.

2,291 stars have been observed three times in each element, 535 twice, and 117 once only. 138 stars whose positions were not considered to have been sufficiently well determined in the earlier zones have been reobserved during the year.

The planet *Mars* and comparison stars have been observed 38 times on the meridian for a fresh determination of the solar parallax, and a few northern stars have been observed as a

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check upon the accuracy of the tabular refractions adopted in our reductions.

The weather has been rather unfavourable for observing during a considerable part of the year, and some difficulty has been experienced in making the *Mars* observations without an interruption of the proper work of the year.

The volume for 1875 is printed, and a copy has been forwarded to the Society. The Catalogue contains positions of 1,930 stars.

The observations made during the year 1876 are reduced to mean places, but the ledgers are not yet formed.

The reductions of the large mass of observations made in 1877 are complete to apparent places, but some of the work still requires examination.

A working catalogue has been prepared for the year 1878. It is hoped it may be cleared off within the year, and thus bring to a completion the reobservation of the stars in Lacaille's zones and the stars in Brisbane's Catalogue of not less than the seventh magnitude. The observations made 1871–1875 have been reduced to a common epoch, and form a catalogue of more than 3,700 stars. The precessions and secular variations have been compared for the epoch of the Catalogue, and the whole arranged for press. But although the Catalogue includes the southern stars from 145° to 180° N.P.D., it has not been considered desirable to send it to press before the observations to be made during the present year and those made in 1876 and 1877 can be included with it in one general catalogue.

A Catalogue has been formed from the observations made here from 1834 to 1840, both inclusive, and it is now being passed through the press. The epoch adopted is 1840. Some progress has been made in the reduction of the observations made about 1850. It is intended to form another Catalogue for that epoch from the observations made about that period.

Time-balls are dropped at one o'clock each day at the Observatory, Simon's Town, Port Elizabeth, and Kimberly, and a gun is fired in Cape Town. At noon a ball is dropped in the Cape Town Docks for the use of the shipping, and a private ball, erected by a Cape Town clockmaker, is dropped by a secondary current. A clock has been established at Port Elizabeth, which is regulated daily after a comparison with the mean solar clock of the Observatory. These arrangements have generally worked well, and through them accurate Cape mean time is distributed over a large portion of the Colony. In long circuits, like those in use, failures will occasionally take place through breakage or carelessness, but such failures are not frequent. The meteorological observations made in the year 1876 have been printed, together with some observations made in different parts of the Colony, in a Report of the Cape Meteorological Committee for the year 1876.

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Melbourne Observatory.

The following is a summary of the work on which the Melbourne Observatory has been engaged during the past year.

The Transit Circle has been chiefly employed in observing, besides the usual fundamental stars, the stars of the B.A. Catalogue, included between 120° and 140° of N.P.D., *Mars* and the comparison stars selected by Prof. Newcomb, and the stars chosen by Mr. Gill for heliometric comparison with *Ariadne* and *Mars* at the island of Ascension. Very favourable weather was experienced during the Opposition of *Mars*, so that out of ninety-four nights observations of the planet were obtained on sixty-seven.

Differential measures of D'Arrest's Comet were obtained on five mornings in the month of June, with the north or south equatoreal.

The Great Reflector has been kept constantly at work, except during occasional repairs, the most important of which was caused by the fracture of the clamp ring of the declination axis.

The observations have consisted, for the most part, of the regular revision of the nebulæ observed and figured by Sir John Herschel, and the search for the satellites of *Mars*, which was unsuccessful, although there can be little doubt that one of them was seen for about an hour on October 16; cloudy weather, however, prevented satisfactory verification. The telescope continues in excellent working order.

With the 8-inch Refractor, besides occasional planetary observations, measures of southern double stars, especially the known or suspected binaries, have been continued.

Sun-pictures have been obtained on every clear day with the Dallmeyer Photoheliograph.

The meteorological work of the Observatory has been carried on as usual, and in January last a system of intercolonial weather telegraphy was commenced, which promises to be a considerable benefit to the public; the results of the telegrams are arranged in a weather bulletin, which is lithographed at the Observatory each day at noon, and distributed shortly after in Melbourne, where it is exposed at the telegraph and shipping offices, and in numerous other public places. The bulletin shows the state of the weather and sea along the south and east coasts, as well as the state of the weather inland at nine o'clock in the morning. No attempt has yet been made to publish the information in the form of a weather-chart.

It became necessary to erect a new house for the magnetographs, and whilst this was being done the magnetographic work had to be suspended from August 27 to November 28. The new magnet-house comprises a basement-floor for the magnetographs, where a fairly even temperature can be maintained; above this a photographic room, and chambers for the thermograph, electro-

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graph, and barograph—so that all the photographic recording instruments of the Observatory are now housed in one separate building.

The usual monthly determination of the three elements of absolute force of terrestrial magnetism has been carried on regularly, and the results of these, as well as of the meteorological work of the colony generally, are published in the *Monthly Record of Meteorology, &c.*, and in an annual volume entitled *Results of Observations in Meteorology and Terrestrial Magnetism.*

As regards the publication of the work of the Observatory, the last volume issued contained the observations for 1870; those for 1871–1876 are nearly ready for placing in the printer's hands.

The drawings of all the nebulæ obtained with the Great Reflector have been lithographed, and will shortly be issued, with the necessary descriptions and details of observation. In the lithographs the nebulæ will appear white on a black ground.

The Observations of *Mars* at the recent opposition are now in course of preparation for the printer, as it is intended to publish them *in extenso* in a pamphlet form for immediate distribution.

The last volume of *Meteorological Results* issued was for 1875.

The Zone Observations of Stars down to the 10th magnitude, between the parallels 150° and 160° N.P.D., are nearly complete, and it is intended to publish them as soon as possible; they already comprise a list of over 48,000 stars.

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Notes on some Points connected with the Progress of Astronomy during the Past Year.

Researches on the Motion of the Moon.

Several years ago Professor Newcomb pointed out that Hansen's *Tables de la Lune* do not satisfactorily represent the terms of long period in the motion of the Moon, and that it was only by sacrificing for the period antecedent to 1750 the agreement between the observed and tabular places of the Moon that Hansen was able to make his tables represent the Moon's place during the interval 1750–1860. It is known that Hansen secured this agreement between observation and theory by giving empirical values to the coefficients of the celebrated terms of long period which were discovered by him to be produced by the action of the planet *Venus*. Although the value thus obtained for the coefficient of one of these terms has been confirmed by Delaunay and Newcomb, the value of the other term has been reduced by them to a small fraction of the value assigned to it by Hansen. Sir G. Airy has shown that the effect of this correction to Hansen's value of one of his terms of long period is to leave serious discordances between the observed and theoretical places of the Moon even during the epoch 1750–1850. Moreover, of late years Hansen's Tables show considerable and increasing deviations from the observed place of the Moon, the tabular errors being now over twelve seconds of arc. It is becoming imperatively necessary to secure greater accordance between the tabular and observed places of the Moon.

With the view of elucidating this important subject, Professor Newcomb undertook the reduction of every observation of an eclipse or an occultation previous to 1750 which appeared to be worthy of confidence and calculated to throw any light on the question of change in the Moon's mean motion. This work Professor Newcomb has now completed, and it is understood that it is in course of publication. In the meantime Professor Newcomb has published a summary of the results of his investigation in an important paper in the *American Journal of Science* for November 1877. The data made use of by Professor Newcomb in his investigation consists of eclipses quoted by Ptolemy in the *Almagest*, of eclipses observed by the Arabian astronomers during the ninth and tenth centuries, and of the occultations and eclipses observed by European astronomers in the seventeenth and earlier half of the eighteenth centuries. From these observations Professor Newcomb obtains the correction to the place of the Moon derived from Hansen's Tables, and these corrections form the basis of his research. They show that, besides other

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corrections, the empirical value of the secular acceleration of the Moon which was employed by Hansen ought to be decreased from 12″·17 to 8″·41, thus bringing it into greater accord with the theoretical value of the acceleration discovered by Laplace, which is 6″·17. Even after being thus corrected, Hansen's tabular places show ontstanding errors of such considerable magnitude that Professor Newcomb arrives at the conclusion that the tables still fail to represent the observations. Where are we to look for the source of this failure?

Professor Newcomb draws attention to the fact that these discordances can be explained on the hypothesis which he advanced some years ago, that there may exist inequalities in the period of rotation of the Earth, and he remarks: "For some years past it has seemed to me that this was the most probable hypothesis on which to explain the deviations in question. It was evidently a most unwelcome one, for, granting its truth, it would be no longer possible to predict the apparent motion of the Moon, since the changes in the rotation of the Earth could not be expected to follow any determinate law. It is therefore extremely gratifying to find that the comparisons we have just given lead to the hope that these deviations may, after all, be due to the action of some of the bodies of the solar system." From a critical examination of these discordances Professor Newcomb shows the extremely curious fact that the deviations since 1625 can be completely represented by merely changing the sign of the constant part of the argument of the term of long period discovered by Hansen to be produced by the direct action of *Venus*, and which depends on eighteen times the mean motion of *Venus* less sixteen times the mean motion of the Moon. This most curious fact is, in Professor Newcomb's opinion, a mere accidental coincidence, as he shows that there cannot be any error in the sign of this part of the term discovered by Hansen.

Professor Newcomb concludes his interesting paper with the following remarks: "Although Hansen, Delaunay, and myself have all arrived at the same result for the value of the term in question, I cannot confidently say that the result is complete. In all these computations the terms of the second order due to the mutual attraction of *Venus* and the Earth are neglected. It is evident that, in consequence of this mutual attraction, the direct action of *Venus* on the Moon is different from what it would be if each planet moved in its elliptic orbit. It may be that this difference is sensible in terms of so high an order as those under consideration. I have actually computed the additional terms in $\frac{1}{\Delta^3}$ (Δ being the distance of *Venus* from the Earth) which arise in this manner, and which depend on the argument 18V − 16E. The result is that the values of the several parts which make up this term are quite comparable with those of the elliptic terms which depend on the same argument; but these

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coefficients destroy each other in taking the sum. I have, however, always regarded my computations on this subject as incomplete, and have, in consequence, never published them. As the case stands, the marked agreement between theory and observation which is produced by the introduction of this empirical term seems to me such as to warrant its provisional use until a more careful investigation of the subject can be made."

Coincidently with the publication of this paper by Professor Newcomb, a new contribution to this subject was made by Mr. Neison, who has announced the discovery of what he considers to be probably a new and important term of long period in the mean motion of the Moon, arising from the direct perturbation of the planet *Mars*. This new term, it is stated, fully accounts for the discordance between theory and observation since 1770, so that its existence is to this extent confirmed by observation; but the exact value of its coefficient is regarded by Mr. Neison as being very uncertain. The determination of the value of this new term is even more laborious and difficult than those due to *Venus*, from the great eccentricity of the orbit of *Mars*. If Mr. Neison be able to confirm the results provisionally arrived at by him, an important step will have been made in reconciling the theory of the Moon with observation.

Mr. Neison has discovered a new lunar inequality which is due to the action of the planet *Jupiter* and is exactly analogous in form to the lunar evection, the mean longitude of *Jupiter* taking the place of that of the Sun in the expression of its argument.

This inequality may be represented by giving small periodic changes to the eccentricity and longitude of the perigee of the Moon's orbit, these changes running through all their phases in a little more than 17 years.

Professor Newcomb had already noticed these changes, and had determined approximately their magnitude and period by a comparison of Hansen's Tables with about 20 years observations made at Greenwich and Washington, but it was reserved for Mr. Neison to trace these inequalities to their cause.

Accompanying these inequalities and having the same period, Mr. Neison has also found an inequality in the Moon's mean longitude amounting to about 2″ at its maximum.

Mr. Hill's Paper on the Motion of the Moon's Perigee.

A remarkably able Paper has been recently published by Mr. G. W. Hill, assistant in the office of the American *Nautical Almanac*, on the part of the motion of the lunar perigee which is a function of the mean motions of the Sun and Moon.

When the eccentricity of the Sun's orbit is neglected, it is

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well known that the equations of the Moon's motion admit of a first integral, due to Jacobi.

Employing this integral, and supposing the inequalities independent of the eccentricity of the Moon's orbit to be already known, Mr. Hill shows that the determination of the inequalities which involve the first power of the eccentricity may be reduced to the solution of a linear differential equation of the second order, which is of a very simple form. This equation is equivalent to an infinite number of algebraical linear equations, and Mr. Hill, by a very elegant method, shows how to develop the infinite determinant corresponding to these equations in a series of powers and products of the small quantities forming their coefficients, the coefficient of each term being obtained in a finite form. By equating this determinant to zero, an equation is obtained which gives directly, without the need of successive approximations, the motion of the Moon from the perigee during half of a synodic month.

The investigation takes into account all terms of lower orders than the sixteenth, considering the ratio of the mean motion of the Sun to that of the Moon to be a small quantity of the first order. The ratio of the motion of the perigee to that of the Moon thus obtained is true to twelve or thirteen significant figures. The author compares his numerical result with that deduced from Delaunay's analytical formula, which gives the ratio just mentioned in a series of powers of m, the ratio of the mean motion of the Sun and Moon. The convergence of the series is so slow that, although Delaunay carries the series as far as m^9, the terms calculated do not suffice to give the first four significant figures of the result correctly.

Professor Adams' Paper on the Motion of the Moon's Node.

In the course of his researches on the lunar theory, Professor Adams had fallen on the same infinite determinant as that considered by Mr. Hill, and had evaluated it in a very similar manner.

By the employment of this determinant, Professor Adams has found the part of the motion of the Moon's node which is a function of the mean motions of the Sun and Moon. This is a much simpler problem than the determination of the motion of the perigee, since the differential equation to be solved is in this case given directly by the equations of motion in the form to which Mr. Hill so skilfully reduces the equation in the other case. The ratio of the motion of the node to that of the Moon thus obtained is true to ten or eleven significant figures. In this case the analytical formula converges much more rapidly than in the case of the perigee, so that although, like Plana,

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Delaunay only carries the approximation as far as m^7, the error of his result only amounts to a unit in the sixth significant figure.

The Astronomer Royal's Numerical Lunar Theory.

In the Appendix to the *Greenwich Observations* for 1875, the Astronomer Royal has exhibited in a tabular form the present state of his Numerical Lunar Theory. In this method the algebraical expressions for the Moon's coordinates which have been obtained by Delaunay are first converted into numbers and then substituted in the differential equations of motion. In the next place the symbolical variations are found which would be introduced into these equations by symbolical variations of every numerical result of Delaunay's calculations which can be supposed liable to error. Then, by comparing the coefficients of corresponding terms in these equations, subordinate equations will be formed from which the corrections of every one of Delaunay's terms may be found.

In the Tables already published the substitution of Delaunay's value of the Moon's coordinates in the parts of the equations of motion which do not involve the disturbing forces has been completed, and the symbolical variations of the same parts of the equations have been found in terms of the symbolical variations of the Moon's coordinates.

In the past year, the Astronomer Royal states, considerable progress has been made with his Lunar Theory. The symbolical corrections of the numbers calculated from tabular elements are computed and are in the press; the perturbations are essentially completed, and some parts which appeared to require revision are under examination.

Continuation of Damoiseau's Tables of Jupiter's Satellites.

Damoiseau's Tables, which terminate with the year 1880, have been extended to the end of the century by Dr. D. P. Todd for the American *Nautical Almanac*, without any change in the fundamental formulæ. The time has hardly yet come for the formation of entirely new tables, and it was thought better not to introduce any provisional corrections, which might cause inconvenience to the future investigator. In the course of his computations Mr. Todd has detected over two hundred and forty errata in Damoiseau's Tables.

The Appendix to the *Nautical Almanac* for 1881 contains a continuation, by Professor Adams, of Damoiseau's Tables of

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Jupiter's Satellites, which in the original form do not extend beyond 1880. In this continuation the perturbations of *Jupiter* are taken from Leverrier's Tables of the planet, instead of from Bouvard's. The long inequalities of the satellites have been recomputed, the values given by Damoiseau being incorrect in consequence of his having omitted to take into account the modification of these inequalities in the case of the first three satellites caused by their mutual action. Professor Adams has also corrected some small errors and inconsistencies in Damoiseau's values of the mean arguments of the equations.

General Hannyngton's Table of Haversines.

In the course of the year an extensive seven-figure Table of Natural and Logarithmic Haversines, by Major-General Hannyngton, for use in the *Nautical Almanac* office, has been issued by the Government. The name *haversine*, now commonly used in works upon navigation, is an abbreviation of *half versed sine*, viz. the haversine of x is equal to $\frac{1}{2}(1-\cos x)$, that is to $\sin^2 \frac{1}{2}x$. In 1805 James Andrew published an octavo work, entitled *Astronomical and Nautical Tables, with Precepts*, which contained a table giving the value of $\sin^2 \frac{1}{2}x$ from $x=0°$ to $x=120°$, at intervals of 10″, to seven places, but the work seems to have attracted little notice, and no reference is made to it in the ordinary treatises on navigation. A copy of Andrew's work having by chance fallen into the hands of Major-General Hannyngton, he improved and rendered more complete the arrangement of the table, and then brought it to the notice of the Superintendent of the *Nautical Almanac*, offering at the same time to add the corresponding logarithms for each second of time up to 12^h. Mr. Hind expressed his approval, and the table has been printed at the public cost. The whole of the table was, in effect, recomputed by General Hannyngton by a partly mechanical method, a combination of two arithmometers being employed.

A table of haversines is of use in the solution of spherical triangles when two sides and the included angle are given, and in many other problems in spherical trigonometry.

General Hannyngton's table gives logarithmic haversines for every 15″ from 0° to 180°, and natural haversines for every 10″ from 0° to 180°, to seven places of decimals, except near the beginning of the table, where the logarithms are only given to five or six places. The arguments are expressed also in time, and proportional parts are added. The table occupies 327 folio

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pages. The utility of the work would be increased by the addition of a suitable introduction explaining the uses of haversines. The table, which is a valuable one, may admit of application in various ways besides those for which it was undertaken.

Reductions of the Observations of the Transit of Venus.

The work has gone on steadily all the year, under the immediate superintendence of Captain Tupman. There has been one regular computer within the Observatory, and several external when required.

The heavy longitude work was brought to a conclusion early in the year. The following are the adopted positions of the various stations:—

	Latitude.				Longitude.			
	°	′	″		h	m	s	
Mokattam, Cairo	30	1	46·3	N.	2	5	6·6	E.
Suez	29	58	22·7	N.	2	10	13·4	E.
Luxor	25	41	40·	N.	2	10	33·1	E.
Honolulu	21	17	56·3	N.	10	31	27·3	W.
Kailua	19	38	25·	N.	10	24	2·7	W.
Waimea	21	57	13·	N.	10	38	41·3	W.
Point Venus, Rodriguez	19	40	21·4	S.	4	13	43·0	E.
Hermitage „	19	44	47·	.	4	13	46·6	E.
Point Coton „	19	40	54·	S.	4	14	0·0	E.
Burnham, N.Z.	43	36	48·1	S.	11	29	12·0	E.
Observatory Bay, Kerguelen's	49	25	11·5	S.	4	39	34·3	E.
Supply Bay „	49	30	56·0	S.	4	39	6·5	E.
Thumb Peak „	49	31	1·	S.	4	40	42·	E.

The tabular distances of the centers of the Sun and *Venus* affected by parallax and the effect of small variations in the tabular elements have been computed for nearly all the telescopic observations, micrometric measures, and photographs received at the Royal Observatory.

A report has been presented to Parliament containing a detailed statement of the telescopic observations of contact,

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made at the British Government observing stations, and a preliminary mean Solar parallax derived from them by comparison of similar phases at different stations; and a more general solution of all the equations derived from them, not in anyway depending upon the above method, was communicated to the Society by the Astronomer Royal, and was printed in the *Monthly Notices* (vol. xxxviii. p. 11.). The results were as follows:—

$$\begin{aligned}
\text{Mean Solar Parallax} &= 8''\cdot754 - \cdot0081\,(d\mathrm{R}-dr) - \cdot0038\,\beta + \cdot0119\,\zeta \\
d\,\mathrm{RA} &= +5\cdot72 + \cdot313\,(d\mathrm{R}-dr) - 1\cdot250\,\beta + \cdot937\,\zeta \\
d\,\mathrm{NPD} &= -5\cdot02 - 1\cdot097\,(d\mathrm{R}-dr) + \cdot298\,\beta + \cdot800\,\zeta
\end{aligned}$$

where $d\mathrm{R}$, dr are the corrections required to the tabular semidiameters, and β and ζ the small quantities (measured in arc along the line of centers) between the limbs at the phases called β and ζ, by which they differ from true contact.

Remembering that the phase ϵ at *Egress* probably corresponds to β at Ingress, and that

$$\zeta = \epsilon - 0''\cdot72$$

we have

$$\begin{aligned}
\text{Mean Solar Parallax} &= 8''\cdot745 - \cdot0081\,(d\mathrm{R}-dr) - \cdot0038\,\beta + \cdot0119\,\epsilon \\
d\,\mathrm{RA} &= +5\cdot05 + \cdot313\,(d\mathrm{R}-dr) - 1\cdot250\,\beta + \cdot937\,\epsilon \\
d\,\mathrm{NPD} &= -5\cdot60 - 1\cdot097\,(d\mathrm{R}-dr) + \cdot298\,\beta + \cdot800\,\epsilon
\end{aligned}$$

The actual value of β and ϵ must be about $1''$, probably less, hence

$$\begin{aligned}
\text{Mean Solar Parallax} &= 8''\cdot753 - \cdot0081\,(d\mathrm{R}-dr) \\
d\,\mathrm{RA} &= +4\cdot74 + \cdot313\,(d\mathrm{R}-dr) \\
d\,\mathrm{NPD} &= -4\cdot50 - 1\cdot097\,(d\mathrm{R}-dr)
\end{aligned}$$

Most of the observations of the phase ζ at *Egress*, made in Egypt, India, Australia, and the Cape of Good Hope, have been computed and the similar phases combined. The results are shown in the following table in which the numbers immediately following the names of places are:—1°. The number of observations used; 2°. the increase of distance of centers corresponding to an increase of $\frac{1}{100}$th of the parallax; 3°. the same for an increase of 1^s in the time.

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Mean Solar Parallax by comparison of similar Phases at INTERNAL *Contact at Egress.*

Place or District.	No. of Obs.	Parallax factor.	Time factor.	Mokattam and Suez. 4 −·202 +·034 ω		Luxor. 3 −·191 +·034 ω		Abbasseyeh. 1 −·202 +·034 ω		Roorkee. 1 −·196 +·036 ω		Madras. 1 −·145 +·034 ω		Mussoorie. 1 −·197 +·035 ω		Mean. ω	
		"	"	"		"		"		"		"		"		"	
Sydney, N.S.W.	5	+·128	+·033	8·76	24	8·78	19	8·78	9	8·81	9	8·91	6	8·61	9	8·768	77
Goulburn, N.S.W.	2	+·128	+·033	8·88	14	8·90	12	8·92	7	8·92	7	9·05	5	8·73	7	8·891	54
Woodford, N.S.W.	1	+·126	+·033	8·82	9	8·79	8	8·81	5	8·83	5	8·95	4	8·64	5	8·801	36
Victoria	3	+·130	+·032	8·91	19	8·90	15	8·90	8	8·93	8	9·05	6	8·74	8	8·900	64
Adelaide, S.A.	3	+·112	+·033	8·77	16	8·79	14	8·79	7	8·82	7	8·94	5	8·61	7	8·778	57
Cape Observatory	2	+·058	+·031	8·50	9	8·51	5	8·52	5	8·55	4	8·63	3	8·30	4	8·495	31
Port Elizabeth, C.G.H.	1	+·051	+·031	8·44	5	8·41	4	8·43	3	8·46	3	8·51	2	8·20	3	8·385	21
			Mean	8·774	96	8·780	77	8·780	45	8·804	45	8·918	31	8·597	45	8·770	340

The weight $\omega = (\text{sum of parallax factors})^2 \times \frac{\text{product of No. of Obs.}}{\text{sum of No. of Obs.}}$.

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It should be remarked that of the Australian stations the only well-established longitude is that of Melbourne. There exist at the Royal Observatory no data for correcting the assumed longitudes of places in the other Australian colonies by referring them to Melbourne Observatory. Mr. Russell, the Government Astronomer at Sydney, N.S.W., stated that the difference of longitude between that place and Melbourne, obtained by telegraph, differed considerably from the difference between their independent absolute longitudes obtained by lunar observations.

The general mean, 8″·770, is therefore only provisional. The previous result from *Ingress* was 8″·741, with weight 101. The general result of the comparison of similar phases is, therefore, 8″·76, subject to future correction.

The favourable combinations of the last *external* contact have also been computed, as some astronomers have thought the observation admitted of considerable accuracy. There are no less than seven observations of the last contact in the colony of New South Wales, all made with large instruments, and their extreme discordance is *sixty* seconds of time, or 2″·34 in distance of centers. It is remarkable that no one of these combinations gives a parallax exceeding 8″·70. A possible cause of systematic error suggests itself in that the *Egress* was observed in Australia in the afternoon of a tropical summer, while the Northern Stations had a winter's morning.

The table on the next page exhibits the individual results.

The remarks made above relating to the longitudes of course apply equally to these results. The longitude of the Mexican Station in Japan depends upon fourteen culminations of the Moon, carefully corrected for the errors of the Tables. The propriety of comparing the Mexican contact observations with the others is questionable, as they were made by projecting the Sun's image on a white screen by means of the ordinary eyepiece.

A general solution of the whole of the contact equations will shortly be communicated to the Society.

We have only now to mention the Photographs. No difficulty was found in determining the distortion of the Dallmeyer instruments to the degree of accuracy of ·001 millimeter. It was found to be symmetrical around the centre of the field, and almost identical for all five instruments.

All the computations necessary for combining the measures of the Photographs have been made, but unexpected difficulties have arisen in measuring them. Twenty-four negatives that had been measured by Mr. Burton were remeasured by Captain Tupman with the same instrument, and it was found that the mean distance of centres differed from Mr. Burton's by nearly one second of arc, or about $\frac{1}{25}$ part of the parallactic displacement of *Venus*. Experiments are being made with a view to discovering some more certain method of measurement.

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Mean Solar Parallax from the Observations of EXTERNAL *Contact at Egress.*

District.	No. of Obs.	Parallax factor.	Time factor.	Mokattam and Abbasseyeh. 4 −·215 +·034	Luxor. 2 −·205 +·034	India. 4 −·187 +·035	Japan (Mexican). 2 −·095 +·033	Mean.
			"	" "	" "	" "	" "	" "
N.S. Wales	7	+·156	+·033	8·65 35	8·67 20	8·58 29	8·47 10	8·613 94
Adelaide	2	+·138	+·033	8·57 19	8·59 12	8·49 14	8·33 5	8·528 50
Victoria	1	+·155	+·032	8·42 11	8·43 9	8·32 9	8·12 4	8·359 33
Cape of Good Hope	4	+·027	+0.31	8·67 12	8·70 7	8·55 9	8·30 2	8·620 30
Mean				8·554 77	8·609 48	8·518 61	8·354 21	8·563 207

The weights have been obtained as before.

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Professor Henry Draper's Researches with reference to the existence of Oxygen in the Sun.

In a paper read before the American Philosophical Society, on July 20, 1877, Professor Henry Draper announced that he had found evidence of the existence of oxygen in the Sun. In some photographs of the solar spectrum which he had taken, he noticed numerous bright lines or bands, which he conceived were not merely interspaces between absorption lines. On comparing the spectrum of a Leyden spark taken in air between terminals of iron and aluminium, he found that the bright lines of oxygen and nitrogen appeared to coincide in position with some of the bright lines of the solar spectrum, while the iron lines of the spark spectrum were coincident with the well-known dark iron lines of the solar spectrum, showing that the two spectra were accurately superposed, wave-length for wave-length. In addition to the coincidence in position of the oxygen and nitrogen lines, Dr. Draper points out the similarity in appearance and relative intensity of the several lines in the two spectra as strongly increasing the probability of his conclusion.

It will be remembered that up to the present time the existence of elements in the Sun similar to those known in terrestrial laboratories has depended on the identity of *some* of the dark lines in the solar spectrum with lines in the spectra of a certain number of metallic elements. Evidence of the existence of most of the common terrestrial elements has been entirely wanting, though the nebular hypothesis seemed to render it probable that the terrestrial elements would be found to exist in every member of the solar system; and, as Dr. Draper remarks, it seemed especially improbable that an element so abundant as oxygen, which forms eight-ninths of the water of the globe, and is stated to form one-third of the crust of the Earth, and one-fifth of the air, should be entirely wanting in the Sun. It seems at first sight difficult to believe that an incandescent gas could exist in the solar envelope whose presence would not be indicated by dark lines in the spectrum, but it has long been known that there exists no dark analogue to the bright D_3 chromosphere line, and that the 1474 Fraunhofer line is relatively very faint as compared with the intensity of the bright 1474 chromosphere and corona lines. Dr. Draper suggests that the law of exchanges which has hitherto been received possibly only holds with regard to a certain number of the metallic elements, and that the presence of other elements in the Sun may be detected by means of bright lines only.

In support of his assertion with regard to the bright lines of oxygen and nitrogen, Dr. Draper has published an albertype copy of a photograph of a part of the solar spectrum compared with the spectrum of a Leyden spark in air. The two spectr

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are made slightly to overlap, so that the coincidences may appear more marked. In describing the photograph, Dr. Draper remarks that "No close observation is needed to demonstrate to even the most casual observer that the oxygen lines are found in the Sun as bright lines, while the iron lines have dark representatives. The bright iron line at G (4307), on account of the intentional overlapping of the two spectra, can be seen passing up into the dark absorption line in the Sun. At the same time the quadruple oxygen group alone is almost sufficient to prove the presence of oxygen in the Sun, for not only does each of the four components have a representative in the solar spectrum, but the relative strength and general aspect of the lines in each case are similar." "I do not think," continues Dr. Draper, "that, in comparisons of the spectra of the elements and Sun, enough stress has been laid on the general appearance of lines apart from their mere position. In photographic representations this point is very prominent. The fine double line at 4319, 4317, is plainly represented in the Sun. Again, there is a remarkable coincidence in the double line at 4190, 4184. The line at 4133 is very distinctly marked. The strongest oxygen line is the triple one at 4076, 4072, 4069, and here again a fine coincidence is seen, though the air spectrum seems proportionally stronger than the solar. But it must be remembered that the solar spectrum has suffered from the transmission through our atmosphere, and this effect is plainest in the absorption at the ultra-violet and violet regions of the spectrum."

Dr. Draper found that the research proved more tedious and difficult than would be supposed, on account of the difficulty of securing good photographs of the spark spectra; but by means of a dynamo-electric machine, driven by a two-horse power petroleum engine, he at length succeeded in obtaining uniform sparks and photographs, and he now hopes to continue his researches with regard to the other bright lines of the solar spectrum.

M. Janssen's Photographs of the Sun.

M. Janssen has recently presented to the Astronomical Society a glass photograph of the Sun, about twelve inches in diameter, showing a remarkable structure of the photosphere, which appears to change from hour to hour. The photographs are taken by M. Janssen, at his Observatory at Meudon, with an instrument somewhat similar to the Kew photoheliograph, though upon a larger scale. The secondary magnifier has been contrived with the assistance of M. Prazmowski, so as to give a very sharp image for the rays that are most strongly photographic, and, with short exposures, photographs showing the peculiar structure are obtained. In the last volume of the *Annuaire* of the

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Bureau des Longitudes a paper proof is given, taken from an enlargement of a portion of one of these photographs. The structure appears to consist of white rounded spots, separated by darker interspaces. The whole surface has also a mottled appearance, which seems to be caused by areas where the structure is rendered indistinct or clouded.

The Suspected Intra-Mercurial Planet.

Owing to an announcement made by M. Le Verrier to the effect that the suspected Intra-Mercurial Planet *Vulcan* might possibly pass across the Sun's disk on or about March 22, 1877, a strict watch was undertaken by several astronomical observers. The lateness of the publication of M. Le Verrier's announcement (*Monthly Notices*, xxxvii., p. 229–230) rendered it impossible to give notice by ordinary postal communication to observers at stations in distant longitudes, but at the representation of the Astronomer Royal the Admiralty liberally sanctioned the expense of telegraphic notices to several stations in India and Australia, and at nearly all a most careful watch was maintained on March 21, 22, and 23.

The total failure of the observations as well in the opposite hemisphere as in our own renders it almost certain that no such object crossed the Sun's disk at the predicted time.

It may be well here to mention that a search was made for intra-Mercurial planets during the total solar eclipses of July 18, 1860, August 18, 1868, and August 7, 1869, but with the exception of a point of light which was observed during the eclipse of 1869, and which has since been satisfactorily identified as the star π^1 *Cancri*, no such body was observed. The Earth could only be near to the line of nodes of the planet observed by Lescarbault towards the end of March or the end of September, and it cannot, therefore, be supposed that during any of the above eclipses Lescarbault's planet was hidden behind the Sun at superior conjunction.

Discovery of Minor Planets.

Since the last anniversary to February 8, 1878, eleven minor planets have been discovered, making the total number 183. The following table contains the number and names of the planets when known, the date and place of discovery, and the name of the discoverer.

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Name and Number of Planet.		Date of Discovery.	Place of Discovery.	Name of Discoverer.
(173)		1877 August 2	Marseilles	Borrelly.
(174)		,, September 3	Ann Arbor, U.S.	Watson.
(175)		,, October 1	Ann Arbor, U.S.	Watson.
(176)	Idunna	,, October 14	Clinton, U.S.	C. H. F. Peters.
(177)		,, November 5	Paris	Paul Henry.
(178)	Belisana	,, November 6	Pola	Palisa.
(179)		,, November 12	Ann Arbor, U.S.	Watson.
(180)		1878 January 29	Toulouse	Perrotin.
(181)		,, February 2	Marseilles	Cottenot.
(182)		,, February 7	Pola	Palisa.
(183)		,, February 8	Pola	Palisa.

The three planets, (170), (171), and (172), discovered in January and February 1877, included in the tabular statement of newly discovered minor planets in the last Report, have received respectively the names of *Maria*, *Ophelia*, and *Baucis*.

Observations have been made during the year of several apparently new minor planets, but which were afterwards found to be rediscoveries of known planets whose orbits have not been sufficiently well determined. One of these, detected by Dr. Peters on February 6, and announced by him as a new planet, has been recognised as *Antigone*. E. D.

Discovery of Comets.

Five new Comets have been detected during the past year; the news of the first, which was discovered by Mr. Borrelly on February 8, arrived just in time to permit of its being noticed in the last Annual Report. Besides these, M. D'Arrest's Comet has been observed on the occasion of its fifth return to perihelion. The following list has been communicated by Mr. Hind :—

Comets in the year 1877.

g a. Discovered by M. Borrelly at Marseilles, Feb. 8.

g b. By Prof. Winnecke at Strasburg, April 5, A.M.

g c. By Mr. Lewis Swift, April 11, and independently on the 14th, by M. Borrelly at Marseilles, and Herr Block at Odessa, on the 16th.

g d. D'Arrest's Comet of short period: first observation by Prof. Julius Schmidt at Athens on night of July 13, when errors of M. Leveau's ephemeris were + 10ˢ·4 in R.A. and + 55″ in declination. (Schmidt's observation is in the Paris *Bulletin International* of July 31.)

g e. By M. Coggia at Marseilles on Sept. 14, observed to the 29th of Nov. at Leipsic (Paris *Bulletin*, Dec. 11) or (?) later.

g f. By Dr. Tempel at Florence (Arcetri) on Oct. 2.

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In order of perihelion-passages, as the orbits are catalogued, these comets will stand thus :—

1877	I.	Borrelly	Jan. 19.
	II.	Winnecke	April 17.
	III.	Swift	April 26.
	IV.	(D'Arrest's Comet)	May 10 predicted.
	V.	Tempel	June 27.
	VI.	Coggia	Sept. 11.

The parabola appears to suffice for the five new comets of 1877.

The Discovery of the Satellites of Mars.

As the discovery of the satellites of *Mars* by Professor Asaph Hall, of Washington, forms one of the most remarkable episodes in the annals of contemporary astronomy, it seems desirable to give, in his own words, Professor Hall's account of the discovery, which he kindly sent Mr. Glaisher in reply to a private request for more detailed information upon this subject than had been published. Professor Hall's letter is dated December 28, 1877.

"The question whether *Mars* had a satellite or not, although at times occurring to me, I did not seriously consider until the spring of 1877. At that time several things had happened that brought this question prominently before me. Perhaps the principal of these was the discovery, in December 1876, of a white spot on the ball of *Saturn*, which gave me the means of determining the time of rotation of that planet, and taught me how untrustworthy may be the statements of the text books; this had made me ready to doubt the phrase one reads so often, "*Mars* has no Moon." Again, the favourable opposition of *Mars* in 1877 naturally attracted my attention. I then set to work to see what had been done in searching for satellites of this planet. Beginning with the observations of Sir William Herschel in 1783, I found, of course, a great mass of observations of the planet; but since the time of Herschel, who appears to have looked for satellites of *Mars*, no serious search had been made, except by one astronomer—Professor D'Arrest, of Copenhagen. A reference to his work is made by Dr. Klein, in his *Handbook of Astronomy*, vol. i., p. 140, and a more complete statement is given by D'Arrest himself, in the *Astronomische Nachrichten*, vol. lxiv., p. 74. As D'Arrest was an accomplished astronomer and a skilful observer, the fact that he had found no moon on such a favourable occasion as the opposition of *Mars* in 1862 was discouraging; but, remembering the power and excellence of our glass, there seemed to be a little hope left. The southern declination of the planet in the opposition of 1877 was, however,

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against us, and the chances seemed to be in favour of the powerful reflector at Melbourne.

"The search was begun early in August, as soon as the geocentric motion of the planet made the detection of a satellite easy. At first my attention was directed to faint objects at some distance from *Mars*; but all these proving to be fixed stars, I began to examine the region close to the planet, and within the glare of light that surrounded it. This was done by keeping the planet just outside the field of view, and turning the eye-piece so as to pass completely around the planet. While making this examination on the night of August 11, I found a faint object on the following side and a little north of the planet, but had barely time to secure an observation of its position when fog from the Potomac River stopped the work. Cloudy weather intervened for several days. The search was resumed on August 15; but a thunderstorm in the early part of the night had put the atmosphere in a very bad condition, and *Mars* was so blazing and unsteady that nothing could be seen of the object, which we now know was at that time so near the planet as to be invisible. On August 16 the object was found again on the following side of the planet, and the observations of that night showed that it was moving with the planet. On August 17, while waiting and watching for the outer satellite, the inner one was discovered. The observations made on the 17th and 18th put beyond doubt the character of these objects, and their discovery was publicly announced by Admiral Rodgers on the 18th. For several days the inner moon was a puzzle. It would appear on different sides of the planet in the same night, and at first I thought there were two or three inner moons, since it seemed very improbable to me, at that time, that a satellite should revolve around its primary in less time than that in which the primary rotates. To settle this point I watched this moon throughout the nights of August 20 and 21, and saw that there was, in fact, but one inner moon, which made its revolution around the primary in less than one-third the time of the primary's rotation, a case unique in our solar system.

"The satellites were observed by me until October 31, and the Washington observations alone give sufficient data for an accurate determination of their orbits. Good observations of both satellites were made at the Harvard College Observatory with the 15-inch refractor by Professor Pickering and his assistants; and a fine series of observations of both these faint moons was made by Mr. Henry Pritchett, at Glasgow, Missouri, with a 12½-inch Clark refractor. Observations of the outer satellite were made at several observatories in Europe, but, so far as I know, the inner moon, which is the brighter one, but more difficult to observe on account of its proximity to the planet, has not been observed in Europe. At the Harvard College Observatory, besides the observations for position made by Mr. L. Waldo, Professor Pickering and his assistants, Messrs. Searle

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and Upton, made an elaborate series of photometric measures of the brightness of these moons. From the results of these measures Professor Pickering infers that the diameter of the outer satellite is six miles, and the diameter of the inner satellite is seven miles. How nearly correct these values are it is hard to say. Both moons are always inside the glare of light that surrounds the planet, and perhaps the best way of determining their apparent magnitudes is that proposed by Mr. Wentworth Erck, of England, viz. to compare the satellite with a star which is at the same distance from the planet, and then, when the planet has moved away, to determine the magnitude of the star. But there remains the uncertainty of passing from a photometric measure of magnitude to the diameter of the reflecting body. Hence it seems probable that a considerable degree of uncertainty will remain concerning the real size of these satellites. Their apparent stellar magnitudes may be inferred with tolerable accuracy from the apertures of the telescope with which they were seen. The outer satellite was seen by Professor Eastman, with the 9·6-inch refractor of the Naval Observatory, on August 21, and again by Professor Eastman and myself, with the same telescope, on August 28. In this telescope it was a faint object, but distinctly seen. The night of August 28 was the finest, at Washington, of the opposition; and in the 26-inch refractor both the satellites appeared remarkably bright, and both were measured with ease with *Mars* in the field of view. The outer satellite was seen again, with the 9·6-inch telescope, by Mr. H. M. Paul, assistant in the Naval Observatory, on October 1, and by using a magnifying power of 430 Mr. Paul was able to see the satellite with *Mars* in the field of view. Both satellites were seen at several places in this country with Clark telescopes of 12 inches aperture; and in England the outer satellite was seen with a 7-inch Clark glass by Mr. W. Erck. If we adopt Argelander's scale of magnitudes, the smallest star visible in a 9·6-inch refractor is 14·1, and in the 7-inch glass 13·4. Allowing therefore for the glare of light around *Mars*, I think that at opposition we may assume that the outer satellite at its elongation was of the 12th magnitude. The inner satellite must be essentially brighter than the outer one, since I was able to observe it when it was less than 8″ distance from the limb of the planet; while I could not observe the outer satellite at a distance of less than 25″. Assuming the outer satellite to have a diameter of six miles, the angle subtended by this diameter on September 26, the date of Mr. Erck's last observation, was 0″·032; and on the date of Mr. Paul's observation this angle was 0″·031. At the distance of our own Moon this angle, 0″·031, would correspond to a distance of 187 feet on the Moon's surface. Hence, if we assume that the diameter of six miles is nearly correct, it appears that the proposition of a German astronomer to establish on the plains of Siberia a system of fire signals for communicating with the inhabitants of the Moon is by no means a chimerical project.

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"I am now collecting and reducing the observations of these satellites preparatory to computing orbits. But the elements of these orbits are approximately known. Both satellites move nearly in the plane of the equator of *Mars*, and both orbits are probably very nearly circular.

	h	m	
The period of the outer satellite is	30	18·0	M. T.
" " inner "	7	39·5	"

"The angular distance of the outer satellite from the centre of *Mars*, at the distance from the Earth of unity, is 32″·5; and that of the inner satellite is 13″·0. At the time of opposition their distances were 85″ and 34″, and occurred when the satellites were in the angles of position of 72° and 252°. The mass of *Mars* which results from the above period, and distance of the outer satellite, which are more exactly known than the corresponding quantities for the inner satellite, is

$$m = \frac{1}{3054000},$$

a value which probably will not be much changed by future calculations. If we adopt Bessel's value for the angular diameter of *Mars* at the distance unity, viz. 9″·328, and assume that the Earth's distance from the Sun is 93,000,000 miles, the diameter of *Mars* is 4,200 miles, the distance of the outer satellite from the surface of *Mars* is 12,500 miles, and the distance of the inner one is 3,760 miles.

"The peculiar appearance of these two moons to an inhabitant of *Mars* is evident on the slightest consideration. On account of the rapid motion of the inner moon it will rise in the west and set in the east, and, meeting and passing the outer moon, it will go through all its phases in about eleven hours. By observing the culminations of this moon the astronomers of *Mars* have a very exact method of determining Martian longitudes, since, instead of the factor 29, which in the case of our own Moon multiplies the error of observation, the Martians will have a factor less than $\frac{1}{2}$. However, one cannot doubt that the Martian astronomers have their own troubles; perhaps a dense atmosphere and an enormons refraction hinder the accuracy of their work, and the most zealous astronomer might tire of observing a moon that may culminate three times in a day. Moreover, no approximate formulæ would suffice for computing the parallaxes of these moons, which, at the Martian horizon, may amount to eight degrees for the outer one, and to twenty-one degrees for the inner one.

Of the various names that have been proposed for these satellites I like best those suggested from Homer by Mr. Madan, of Eton, viz. *Deimos* for the outer satellite, and *Phobos* for the inner one.

The Secretaries have received communications informing

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them that the outer satellite has been observed in England by Messrs. Christie and Maunder at Greenwich, by Prof. Pritchard at Oxford, by Lord Lindsay and Dr. Copeland at Dun Echt, by Dr. Wentworth Erck, by Mr. A. A. Common, by Mr. Cooper Key, and by Mr. Brett. The inner satellite, they are informed, has been observed by Mr. Maunder twice, at Oxford once, and (query) once by Mr. A. A. Common. Mr. Common's observations have already been published in the *Monthly Notices*, and the other communications will probably appear in a future number.

Drawings of Jupiter.

In answer to a circular sent out by the Committee, referred to in last year's *Annual Report*, 264 drawings of the planet have been received. These were forwarded to Dr. Osw. Lohse, of Potsdam, for examination. He writes: 'The greater number of the drawings which I have received belong to the period of the opposition of 1876. A single year is only a fraction of the time necessary for the study of the periodical appearances which present themselves upon the body of the planet. However, this much can already be recognised by a comparison of the observations of 1869, 1870, and 1871 with those of 1876. The appearance of the planet has since then essentially changed. The arrangement of the belts is not so permanent (*ausgepragt*). There are also wanting those peculiar rows of oval clouds so characteristic of the planet in 1870. In their place there were, in 1876, in this zone only long loops of clouds. In 1877 the equatoreal zone appeared again altered. The broad chief belt was divided into two dark belts by a narrow bright division. The reddish colouring of the dark parts in the neighbourhood of the equator is, in comparison with 1870, considerably faded. Dr. Lohse is preparing a special paper on the subject, in which further details will be given, as also the results of his comparisons of earlier drawings.

The Period of Saturn's Rotation.

On the night of December 7, 1876, Professor Asaph Hall, while observing *Japetus*, noticed a bright spot on the ball of *Saturn*. On the next day letters were sent to astronomers, asking them to assist in observing the appearance of the spot, and with these letters was sent an ephemeris of the spot, computed by assuming the time of rotation of *Saturn* to be $10^h\ 29^m\ 16^s{\cdot}8$ M.T., which is given in nearly all the modern text-books as Sir W.

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Herschel's last and most accurate determination. Although this time of rotation proved to be erroneous, and thus the ephemeris was, perhaps, worse than useless, the spot was observed by several American astronomers; and Professor Hall, on reducing the observations, obtained for the mean time of the rotation of *Saturn* the value $10^h\ 14^m\ 23^s{\cdot}8$, with a probable error of $2^s{\cdot}30$, it being assumed that the spot had no proper motion on the surface of the planet.

Professor Hall states that, as far as he is aware, the only determination of the rotation is that given by Sir William Herschel in the *Philosophical Transactions* for 1794. By observing the different appearances of a quintuple belt from November 11, 1793, until January 16, 1794, Herschel found the time of rotation to be in mean time $10^h\ 16^m\ 0^s{\cdot}4$, and concludes that this cannot be in error by so much as two minutes. At the time of the discovery of the white spot this determination of Herschel's was not known to Professor Hall, and he used the time of rotation as $10^h\ 29^m\ 16^s{\cdot}8$, which is given by nearly all astronomical writers during the present century. Professor Hall suggests that the error may have arisen from the statement in the *Système du Monde*, where Laplace says that *Saturn* rotates in 0·428 of a day, and the ring in 0·437. These, reduced to hours and seconds, give $10^h\ 16^m\ 17^s{\cdot}2$ and $10^h\ 29^m\ 16^s{\cdot}8$. An account of Professor Hall's observations is published in the *Analyst* (March 1877), and also in the *Astronomische Nachrichten*, No. 2146.

The New Star in Cygnus.

A *résumé* was given in the last Annual Report of some of the spectroscopic observations of the new star in *Cygnus* (R.A. $21^h\ 36^m\ 50^s{\cdot}41$ N.P.D., $47°\ 43'\ 21''{\cdot}5$—1876·0) which had been made up to the end of the year 1876. The spectrum of the star, soon after its discovery by Schmidt, consisted of a fairly conspicuous continuous spectrum, with several bright lines. All of which, with the exception of the bright line which is referred to as ζ in the last Annual Report, continued to decrease in brightness, while the continuous spectrum faded out at the same time. The colour of the star, as seen without the spectroscope, when it was first discovered, was yellow or orange. It then became more and more ruddy, up to about the end of January, after which, as it continued to decrease in brightness, a blue tint became noticeable.

Dr. Vogel, in the *Monatsbericht der Königl. Akad. der Wissensch., Berlin*, for May 1877 has given a critical summary of the spectroscopic observations up to end of April 1877. At that date the ζ line was the brightest, many of the other lines which had originally been conspicuous being barely perceptible.

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On the 2nd of September 1877 Dr. Copeland, on examining the star with Lord Lindsay's 15-inch achromatic at Dun Echt, found that its spectrum had been reduced to a single line in the green, the continuous spectrum and the other lines having completely disappeared. The observation was made in the early morning and only a short time remained before dawn, but a hurried observation was made with the single-prism spectroscope, indicating a wave length of ·000512 *mm*, a result which must not be relied upon, as no comparison spectrum was used. The next evening Lord Lindsay made thirteen measures of the wave length of the line with a Grubb spectroscope, giving a dispersion of $3\frac{1}{2}°$ from B to *h*. The wave length obtained was ·0004986 *mm*, a result which may be relied upon, as the day had been spent in making every preparation for the measurements. It will thus be seen that the light of this remarkable star is probably identical in wave length with the nitrogen line ·0004987 *mm*, or with the well-known nebular lines ·0005004 *mm* and ·00049566 *mm*.

Lord Lindsay gives, as limiting wave lengths between which the whole of the monochromatic spectrum of the star lies, ·0005008 *mm* and ·000493·5 *mm*. These limits were determined by bringing a feebly illuminated wire up to either end of the monochromatic image of the star. He remarks that no observer discovering the object in its present state would, after viewing it through a prism, hesitate to pronounce as to its nebular character, but no disk has been detected with powers ranging up to 1,000 or 1,100.

Birmingham's Catalogue of Red Stars.

Observers who have paid attention to red stars will be interested in the monograph published by Mr. Birmingham in the *Transactions of the Royal Irish Academy*,* entitled "The Red Stars Observations and Catalogue." Mr. Birmingham has endeavoured to incorporate the labours of Schjellerup, Webb, Schmidt, Secchi, Huggins, Vogel, and others who have given attention to red stars with his own observations, and has succeeded in compiling a Catalogue of 658 red and orange stars. Schjellerup, who was the first to draw special attention to the red stars, published in 1866 a Catalogue which he had compiled from various authorities. It contained 280 stars, many of which are now of so faint a tint that, in compiling a new catalogue, it is doubtful whether they could be fairly ranked as red stars. Mr. Birmingham has, however, in consideration of the possibility of their having changed their colour, retained them all in his Catalogue.

It has long been known that many of the red stars are variable

* Vol. xxvi., section vii., pp. 249–354.

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in tint as well as in magnitude. Schmidt, who has given some attention to red variables, has remarked that they grow paler as they increase in magnitude, and become more ruddy as they again decrease towards their minimum. Mr. Birmingham's observations also point towards the same generalisation, as if the redness were produced by the interposition of some absorbing matter which blots out a portion of their light. Mr. Birmingham suggests that, if this law is universally true, the reddish naked-eye stars will really be larger than they appear, and thus *Aldebaran* and *Betelguese* may, in fact, be rivals of *Sirius* in size. The variation in tint of some of the red stars is so great as to take them in the course of a few years entirely out of the red class; many change from red to orange, and some even to bluish, or a bluish white tint; thus No. 63 of Schjellerup's Catalogue appeared in February 1863 of a deep red colour, so that he described it in his notes as "rubra," and underlined the word, which was not his usual habit. But on examining the star again in February 1872, it appeared to him, as well as to Mr. Birmingham, as a decidedly bluish or bluish white star. λ *Ophiuchi* was described by Struve in 1834 as yellow; while Secchi, in 1867, set it down as of the same type as *Vega*. And everyone is familiar with the fact that *Sirius* was in ancient times described as red,* while it now appears as a decidedly bluish star; there are also many other instances in the Catalogue in which a decided change of colour seems to have been established. Mr. Birmingham remarks that the periodic variation of tint in many of the red stars goes far to disprove the singular conceit which has been frequently advanced as to the redness of a star showing that it has cooled down, and belongs, on the assumption of the nebular hypothesis, to a later stage of development.

The stars are given in the Catalogue in order of Right Ascension, with their positions brought up to the year 1880. The reference numbers of all the stars in Schjellerup's Catalogues are set out in a separate column, as well as reference numbers of the lists of red stars given by Schmidt† in the *Astronomische Nachrichten*. On the opposite page is a very copious series of references to all the observations which Mr. Birmingham has been able to collect. He refers to the observations of Bessel, D'Arrest, Argelander, Schönfield, Engelmann, Hind, Bond, and to private letters from Burton, Copeland, and others. Mr. Webb has also contributed very largely. We would especially direct attention to his small variable S *Orionis*, which is one of the

* It should be mentioned, however, that Schjellerup, in his preface to Al Sûfi, *Description des Étoiles Fixes*, discusses the evidence with regard to *Sirius* having formerly shone as a *red* star, and comes to the conclusion that it is doubtful. *See* "Knobel's Chronology of Star Catalogues," *Memoirs*, vol. xliii., pp. 6, 7.

† Schjellerup's first Catalogue was published in the *Astronomische Nachrichten*, No. 1591. His second Catalogue was published in the *Vierteljahrsschrift der Astronomischen Gesellschaft*, ix. Band. Schmidt's lists appeared in the *Astronomische Nachrichten*, Nos. 1902, 2009, 2009, 2016, 2032, and 2044.

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most remarkable of its class. Some forty pages are devoted to a discussion of spectroscopic observations of red stars; the various accounts given by different observers, as well as by the same observer at different times, are discussed and many apparent contradictions are pointed out.

This Catalogue will no doubt serve to indicate with greater precision than has yet been attained the general law of distribution of the red stars. The only fact with regard to their distribution alluded to by Mr. Birmingham in his introduction is, that the region of the heavens, including the milky way between *Aquila*, *Lyra*, and *Cygnus*, seems particularly rich in red stars; while Mr. Webb remarks that the other portions of the milky way do not, considering the number of stars, appear to be richer in red stars than other parts of the heavens.

The Literature of Nebulæ and Clusters.

The Smithsonian Institution has just published * a valuable contribution to Astronomical Bibliography, entitled *Index Catalogue of Books and Memoirs relating to Nebulæ and Clusters*, by Prof. Edward S. Holden, of the United States Naval Observatory.

Prof. Holden states that "the design of his Catalogue is to give a full list of books and memoirs relating to Nebulæ and Clusters, and to include references to allied subjects—as the Milky Way, the Nebular Hypothesis, &c., &c.—which may serve to give as complete a knowledge as possible of the first two subjects." The work is subdivided into six sections. Section I., pp. 1–62, is an "Index Catalogue of Books and Memoirs relating to Nebulæ and Clusters," &c., alphabetically arranged by authors. Under the title of each author's name is given a list of all his writings on Nebulæ, arranged by periodicals; and under the title of each publication a reference to all works on Nebulæ and Clusters contained in it. Appended to nearly every reference is a note, more or less extended, giving an indication of the purport of the paper. Prof. Holden has bestowed a great deal of attention on the works of Sir William Herschel. No less than twenty pages of Section I. are devoted to a list, with abstracts, of his papers in the *Philosophical Transactions*. The author states that he has "made them much more full than in other cases, in order to present, if possible, something like an adequate idea of the views of this great man on the subject which is peculiarly his own. This is the more necessary as we have as yet no collection of his works which is generally avail-

* *Smithsonian Miscellaneous Collections, No.* 311. *Index-Catalogue of Books and Memoirs relating to Nebulæ and Clusters etc.* By Edward S. Holden, Washington, Smithsonian Institution, 1877, 8vo., pp. ix–111.

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able (a want which it is hoped may be supplied), and as the earlier volumes of the *Philosophical Transactions* are now quite rare." This portion of the Catalogue alone will be found of great value to astronomers. Prof. Holden has spared no pains to make it a complete epitome of Sir William Herschel's labours in this direction.

The reference to Sir John Herschel's *Cape Observations* is rather meagre, and it would have been an advantage if the author had given more information respecting the several important monographs on Nebulæ contained in that work.

The Nebula of *Orion* has received a vast amount of study at the hands of Prof. Holden. Section II. is devoted to a special bibliography of this object, and contains all observations and researches on it from the period of its discovery by Cysatus in 1619 to the present day.

One interesting fact signalised by the author's researches is the discovery of the 5th star in the trapezium of *Orion*, by Hooke, prior to 1665.

Section III. gives "A List of Books and Memoirs relating to Variable Nebulæ." The attention of astronomers has been much attracted of late years to the remarkable existence of Variable Nebulæ, as evidenced by the one discovered by Mr. Hind in 1852, the curious object in the *Pleiades* discovered by Tempel, later in the observations of southern observers in *η Argûs*, and the author's own researches on supposed changes in the Horseshoe and Trifid Nebulæ. Still more remarkable is the paper by Prof. Winnecke, in the *Monthly Notices* for January 1878, on the "Periodic Variability of the Nebula H II. 278"= G. C. 551.

Sections IV., V., and VI. are devoted to a "List of Figured Nebulæ" (published and unpublished), an "Index to Sir William Herschel's Catalogues of Nebulæ and Clusters," and an "Index to Messier's Catalogue of Nebulæ." This Index-Catalogue is evidently the result of great labour and research, and Prof. Holden is to be congratulated on the successful manner in which he has accomplished his work. It cannot fail to be appreciated by astronomers, and prove of material assistance to them in their researches. The advertisement to it, by Prof. Henry, the distinguished Secretary of the Smithsonian Institution, says: "Whoever attempts the enlargement of the bounds of knowledge in any particular branch of science, in justice to himself, the public, and previous labourers in the same field, should make himself familiar with all that has been previously published on the subject. But information of this kind is so widely dispersed through the journals and transactions of learned societies of all parts of the civilised world, that index-catalogues, or references to authorities, are of the utmost importance to the investigator. In consideration of this fact, the present work has been accepted for publication, after having received the approval of our collaborators in the line of astronomy."

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Mr. Stone's Tables for Star-Constants.

Everyone who has required both the apparent and mean places of small and uncatalogued stars will welcome the appearance of Mr. Stone's new Tables for facilitating the computation of star-constants, contained in an appendix to the *Cape Observations* for 1874, and arranged in a form which enables the computer to obtain in a short time the necessary constants in both elements with all the required accuracy. In the observations of zones of stars, which has been prosecuted with so much success at the Cape Observatory, the labour of computing and examining the large number of star-constants in the ordinary form was found to be very great, and to relieve, in some measure, this pressure on his assistants, Mr. Stone was induced to have these Tables prepared for use in his Observatory. The work consists of two sets of Tables with arguments Right Ascension and Declination, or North Polar Distance, respectively; the epoch for which they are formed being 1880, and the constants adopted being those of Professor C. A. F. Peters. The first Table contains simply the log. sines and cosines of the R.A. to every second of time throughout the twenty-four hours, arranged in a convenient form. These are expanded beyond what is absolutely necessary in order to permit their use in accurate computations of the precession with first differences. The Declination Tables are extended sufficiently to allow the computation of a star-correction to be effected without interpolation, except for stars near the Poles. To find the logarithms of a star-constant it is only necessary to add the corresponding quantities extracted directly from the Tables to the nearest second of R.A. and the nearest minute of N.P.D. or Declination, to within two degrees of the Poles. In tabulating the different quantities, no attention has been paid to the algebraical signs, which can be readily supplied by general rules, according to a table given in the introduction.

The facility with which the constants for any particular star can be calculated by the aid of Mr. Stone's Tables, and the accurate values obtained, enable the computer to cast aside the published constants inserted in the British Association and many other Star Catalogues, which require a considerable correction at the present time, especially for stars near the Poles. Mr. Stone remarks truly that star-constants are only available for accurate purposes for a few years from the epoch for which they have been calculated, and he believes that it has been the practice of most astronomers to employ published star-constants far beyond the periods for which their use can be justified, more particularly in cases where extreme accuracy is aimed at, in accordance with the requirements of modern astronomy. It has, however, been customary at the Royal Observatory hitherto to rely only on the star-constants published in the two last Greenwich combined

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Catalogues. For stars not contained in these two catalogues, and for all of less than 5° N.P.D., whether constants had been previously computed or not, they are always recomputed, and from the beginning of 1878 the use of the star-constants in the Seven-Year Catalogue for 1860 has been discontinued. But for all the practical purposes of an Observatory where the Bessel form of reduction is preferred over that of the Astronomer Royal, in which the principal features consist of an absence of algebraical signs in the logarithmic computation of both star-constants and star-corrections, there can be no doubt that Mr. Stone's Tables will be found of the greatest utility, and that the calculation of a series of star-constants will be much simplified and facilitated by their use. E. D.

Progress of Meteoric Astronomy during the Year 1877.

Falls of meteoric stones have taken place, and some of these bodies have been submitted to analyses, of which some of the announcements made during the past year have been events of importance in the history of aërolitic phenomena. Recorded instances of stonefalls attended by the usual meteoric phenomena occurred on August 16, 1875, in the Cercle de La Calle, in the eastern part of Algeria, at about noon, the stone weighing about 14 oz.; a stone of nearly the same weight fell from the extraordinary meteor of December 21, 1876, in the United States, at Rochester, Ind., at about $8^h\ 43^m$ P.M. local time; and meteorites fell in Missouri, Georgia, and Kentucky, U.S., on the 3rd, 20th, and 23rd of January 1877.* Of these aërolites the stone projected from the meteor of December 21, 1876, fell under circumstances which were the most extraordinary and unusual, and a description of the meteor which produced it, as it was one of the grandest that has for a long time been observed, takes a fitting precedence of all the other meteoric recollections of the past year. The meteor passed, descending gradually from a height of about 75 miles above the southern part of Kansas State west of the Missouri river, across that river and the Mississippi and the States skirting the southern shores of the great American Lakes Michigan and Erie, to a height of between 25 and 30 miles over the western parts of the States of New York and Pennsylvania east of Erie (on the lake of that name), in the latter State. An idea of the immense extent of its visible track may be formed

* The accounts here given of these recent American stonefalls are contained in a paper by Professor D. Kirkwood (read before the American Philosophical Society, March 16, 1877). "On eight meteoric fireballs seen in the United States from July 1876 to February 1877." A note in *Nature* (vol. xvii., p. 232) records the fall of two meteorites in Germany on the 26th of December 1877, at Höhr, Hessen Nassau, near Neuwied, in Coblentz, with the usual detonations.

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from the lengths of the successive portions of it included in the different States over which it passed :—

Kansas	200 miles, at a height of		75 to 50	miles	}	
Missouri	200	„	„	50 to 37	„	} 600 miles.
Illinois	200	„	„	37 to 28	„	}
Indiana	140	„	„	28 to 28	„	}
Ohio	210	„	„	28 to 30	„	} 450 miles.
Pennsylvania and New York	100	„	„	30 to 33	„	}

The calculated heights in this delineation of its course include an allowance for the Earth's curvature and for a deflection of about 30 miles due to gravitation during the whole time of its passage, which cannot, with the observed speed of ten or eleven miles per second, have fallen much short of a minute and a half. The track conforms nearly to the Earth's curvature in the last half, and approaches the Earth in the first half of its length; but a gaining rather than a uniform rate of deflection by gravitation must have rendered its descent throughout more gradual and regular than the calculation shows, since the observed height over Rochester, in its mid-transit over Indiana State, where the aërolite fell, was 38 miles, instead of 28 miles given by the Table; and the meteor descended thence to less than 30 miles above Lake Erie ere a final disruption over the eastern shore of the lake immediately preceded the termination of its visible flight over the States of Pennsylvania and New York, and its course here was still descending to a height of probably not much more than 25 miles at disappearance. With this continued descent from first to last, it is believed that no portion of the meteor's substance passed onwards out of the atmosphere and escaped from the Earth's attraction; but, with the exception of the meteorite at Rochester, no other fragment of its solid substance has yet been recovered; and it is very remarkable that this fragment should have fallen from the meteor fully 300 miles from the furthest point reached by the fireball in its flight, where the splendour of its strangely and magnificently dilapidated nucleus ultimately disappeared. Nor was it at either of these points, but in mid-transit over Illinois State, more than 150 miles before reaching Rochester, that the great concussion occurred which broke up the body of the meteor (at a height of probably more than 40 miles above Peoria, in that State), and which was heard and felt like the shock of an earthquake to a distance from Peoria of at least 200 miles. The time taken by the sound of the concussion to reach Bloomington, in Indiana, 185 miles from this point, was 15^m, timed with a clock; while an explosion at Rochester, a nearer point of the meteor's flight, about 135 miles distant from Bloomington, would, if sufficiently violent to be heard so far, have been audible at Bloomington in eleven minutes. A deflection of the meteor's

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course over Illinois amounting to about 10° towards the east in the middle of its flight is noticeable in the projected path, and is attributed by Professor Kirkwood, who collected and carefully examined all the available descriptions, to the effect of the concussion which the meteor underwent in its passage over Peoria and the central part of the State. An effect equally powerful may at the same time have been produced upon the obliquity of its course, the real height of which certainly diminished very slowly from this point of first intrusion, 40 or 50 miles above the Earth's surface, into the crepuscular atmosphere to the end of its flight, 25 or 30 miles above the south-west part of New York State, 450 miles further on along its track. A similarly deflected path, consisting of three distinct portions, arising from explosions of the meteor (which, however, principally affected its real height and the obliquity of its path to the horizon), was pursued by the great fireball of 1860, July 20, over nearly the same territories of the United States, but escaping at last, over the Atlantic Ocean, unconsumed, from the Earth's atmosphere, with a visible length of path of 1,300 miles. The distinguishing feature of the present fireball was the multitude of fragments into which it burst at its principal explosion, some scores of which, many of them brighter than *Venus* or *Jupiter*, pursued the nucleus in a clustre 3° in width and 20° in length as seen from Bloomington, 135 miles north of which town the meteor's cortége swept eastwards over the State of Indiana in its long-continued flight. Only partial observations of its varying phases could be gained from different points of view along its track, and in respect of precision both of description and of position and duration of the spectacle, much was left to be desired in the accounts; but minor explosions or disruptions of the nucleus occurred both before and after its separation into fragments in mid-course, up to the last of which, just before its disappearance, the flock of fragments appears to have accompanied the principal fireball in a long level flight of nearly 400 miles through the rarest strata of the atmosphere. The sound of the explosion preceded the fall of the meteorite at Rochester, and it appears probable that here, and at other disruptions of the nucleus besides the principal one, audible detonations were perhaps produced. The friable pisolitic nature of the stone, easily crushed to dust and light powder with the fingers, which struck the snow and rebounded when it fell at Rochester, accounts sufficiently for the comminution of the nucleus into lesser fireballs in its course; but the fact of this solitary aërolite only having been seen to descend from such a cluster, at a distance of fully 300 miles from the point of the meteor's final disappearance, still remains an occurrence, unexampled in the appearance of any previous aërolitic fireball, which is very difficult of explanation.*

* A full description and analysis of the stone is given by Dr. J. L. Smith (*American Journal of Science*, vol. xiv., p. 219, September 1877), together with

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Making allowance for the effect of the Earth's zenithal attraction, Professor Newton obtained for the approximate apparent radiant-point, or direction of the meteor's motion relative to the Earth, a position in the eastern and southern point of *Capricornus*, a little south of the ecliptic, about 45° from the antiapex of the Earth's way, so that the meteorite was overtaking the Earth in ecliptic longitude about 90°, with a very small inclination of its orbit plane, at the ascending node, to the ecliptic, with direct motion from a perihelion about 30° behind the node; and the observed velocity of the fireball, estimated both by Professor Newton and Professor Kirkwood at about 10 miles per second, agrees very nearly with the theoretical velocity of 11½ miles per second of a meteorite moving in a parabolic orbit with this radiant-point.

In some specimens of meteoric iron from the Mexican Desert, a district in which even larger meteoric iron-masses than were previously known to exist in it have recently been discovered, and have been partly conveyed to the United States, Dr. Lawrence Smith has detected the presence of a new mineral associated with the meteoric monosulphide of iron termed "troilite."* This is monosulphide of chromium, a mineral

the particulars of the falls and mineral characters of the two other aërolites of January 3 and January 23, 1877, which fell within five weeks of the stonefall in December, in the United States. The aërolite of January 3, 1877, fell at Warrenton, Mo., at sunrise, with a rushing sound, ending with a crash as it struck the limb of a tree 50 or 60 paces from some observers, and broke it, being itself shivered on striking the ground into many fragments, 15 lbs. weight of which that were collected forming only a small part of the entire weight of the stone. It is friable, and resembles the Ornans stone very closely which fell on July 11, 1868. Partly perhaps from approaching daylight, no luminous meteor was seen, and this aërolite appears also to have fallen without any of the ordinarily audible sounds of meteoric detonations! The stone of January 23, 1877, fell from a bright fireball seen generally in Kentucky and Indiana States, U.S., at 4 o'clock P.M., accompanied by detonations. It weighed 13¾ lbs., and it most resembled in internal composition and appearance the Parnallee meteorite. Some descriptions given of the fireball in Professor Kirkwood's paper are not exact enough to trace its real path. No notice of the aërolite of January 20, 1877 (stated by Professor Kirkwood to have fallen in Georgia, U.S.), occurs in the paper of valuable researches on these recent American aërolites presented by Dr. Lawrence Smith.

* "Daubréelite" is the name given by Dr. Lawrence Smith to this new mineral, chromium monosulphide, discovered in the Mexican meteoric irons (*American Journal of Science*, vol. xii., pp. 107–110, August 1876). In a following paper (*Ibid.*, p. 207, September 1876) Dr. Lawrence Smith describes the mineral characters of a previously unexamined aërolite presented to him by Mr. Claywater, of Wisconsin, U.S., in which State the meteorite fell at 9h A.M., on March 26, 1865, only this small fragment of it having been preserved. The agreement which it presents with the unique meteorite of Menö (Alt Strelitz, Mecklenburg; noon, October 1, 1861) is so complete that scarcely a doubt was entertained at first that it was a fragment of that aërolite; but its fall in Vernon Co., Wisconsin, is yet perfectly authenticated. The dates of fall being separated from each other about 183° in longitude on the ecliptic, Professor Kirkwood conjectures (*Nature*, vol. xiv., p. 526) that the orbit which these two meteorites were pursuing belonged in all probability to a common stream. The same community of meteor-system, Professor Kirkwood

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hitherto unknown to terrestrial or celestial mineralogy, and adding another to the natural compounds of sulphur with metallic bases which have only been observed in meteorites. Compared to the copious presence of sulphur in our terrestrial

adds, may probably be conjectured of the two aërolites of Somer Co., U.S. (May 22, 1827), and Utrecht, Germany (June 2, 1843), whose nearly identical composition was remarked by Baumhauer. A very small inclination of their common orbit to the ecliptic would be necessary in this case to account for the considerable difference in the dates of their occurrence; but the inclination of a Claywater-Menö stream is less restricted, and a perihelion midway between the indicated nodes must (from the positions of the two horizons) have lain in southern latitudes between long. 97°, S. lat. 5°, and long. 277°, S. lat. 45°. As the Wisconsin aërolite moved from the north-west to near the south-east horizon, it is easy to choose from these restricted limits the only position of the common orbit, which will approximately satisfy the twofold condition of these stonefalls directed from north-west. The ascending node of the orbit was at Menö (where the aërolite must have come from near the south-west horizon), in ecliptic longitude 8°, its perihelion was in long. 277°, S. lat. about 45° (or greater), and the motion of the meteorites in this stream or common orbit round the Sun was direct. No elements of cometary orbits yet known have any such resemblances to this meteor-track as to render it even distantly probable that they may have had a possible connection with the stream.

Several new meteoric specimens found, but not seen to fall, in recent years, are again described by Dr. Lawrence Smith (*American Journal of Science*, vol. xiii., p. 211, March 1877), with the results of his analyses of them: an aërolite found at Waconda, Kansas, and two aërosiderites or meteoric irons found in Missouri and N. Carolina. The last, found on the surface of a field in Rockingham Co., in 1863, must have fallen there within twenty years previously, since the field was last tilled. It weighed 11 lbs., and it was found to contain in some of the crevices exposed by section, besides Schreibersite, solid chloride of iron, some of which salt, although extremely deliquescent, was preserved. A specimen of it was presented by Dr. Lawrence Smith to the Geological Museum of the Jardin des Plantes, in Paris, the well-known director of which, M. Daubrée, has found it occurring, as an exudation, in several aërolites and meteoric irons, and he has given the name of "Lawrencite" to this newly recognised mineral constituent of meteoric rocks.

In connection with the geological aspect of these inquiries may be noticed the results of deep-sea dredging obtained by the "Challenger" Expedition, and the discovery of peroxide of manganese as an incrustation on shells and other objects in the oozes of the deep ocean-bed. In the *Proceedings of the Royal Society of Edinburgh*, vol. ix., p. 257, the origin of this incrustation is traced by Mr. John Murray, under whose care the specimens were collected and have been examined, to the disintegration of the augitic materials of volcanic tuff and pumice plentiful in the regions where it occurs. Much magnetic dust is contained in this apparently volcanic portion of deep-sea sediment and accumulation; frequent spherules occurring therein Mr. Murray found to contain metallic iron in veins and particles in the same way that native iron occurs in the spherules of meteorites; and the appearance of this metallic iron in the dust and spherules being confined to regions of the deep sea where the rate of deposition has been extremely slow, Mr. Murray concludes that its origin may fairly be ascribed to meteoric sources, and to the gradual subsidence from the atmosphere of meteoric dust. On the other hand, M. Daubrée found that the presence of metallic iron and nickel alloyed with them causes the magnetic polarity sometimes exhibited by grains of native platinum; and that the rocks in which platinum is found dispersed in the Ourals, in New Zealand, and in Borneo resemble greatly by their peridotic and magnesian characters, with frequent presence of chrome-iron, the materials of meteoric stones. (*Annales des Mines*, vol. ix., 1876.)

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metallic ores, the inferior proportion in which this element is found associated with metals in the corresponding compounds occurring in meteorites appears to be in intimate relationship to the sparing quantity of oxygen (uncombined with silicon) and of silica, noticeable as a marked character in the minerals of meteorites, and points apparently, by reasoning which may be derived from our knowledge of the present state of distribution, in ascending height, of elementary vapours on the Sun's surface, to conditions on the surface of those celestial bodies that project aërolites indicative of their smallness compared to our Earth and the Sun, supposing them to have consolidated in the same manner as those masses must have done. The negative proofs hitherto given by the spectroscope of the existence of certain light-atomed elements, as oxygen, nitrogen, carbon, &c., in the Sun's atmosphere, are now replaced by the positive evidence of the abundant presence of oxygen there, furnished by Professor Draper's recent discovery and identification of its bright lines in the solar spectrum. In a paper of much interest on the origin of volcanic disturbances,* Dr. von Tschermak has developed the views most consonant with modern geognostic researches regarding volcanic action on small meteor-yielding planets, to which allusion has already been briefly made in a previous Report.† Classing together all fiery eruptions, from those of intense violence on the Sun to the phases of ordinary activity of terrestrial volcanoes and fumaroles, as liberations of certain confined or included gases from a gradually cooling interior nucleus, a sufficient foundation for the theory of the origin of the Earth's volcanic activity in the evolution of such gases by the act of solidification of some of the interior molten masses is found in the rate at which such solidification must proceed, and in the observed property of many substances (iron, copper, and silver among the metals; sulphur, glass, and especially naturally molten lava when cooling), of secreting in the molten state, and of liberating at the moment of solidification, considerable quantities of various gases and vapours, which produce intumescence of the first incrustations, and vents or craters by which they escape through the thicker hardened shell. Among the kinds of gases readily absorbed and rejected in this manner are hydrogen and carbonic oxide, which are known to be generally occluded in aërolites and meteoric irons. In the molten spheres wherein they first solidified it is therefore probable that these gases were secreted more copiously than in the meteoritic fragments that reach us, and which have been detached from them by eruptions; and the gaseous generation of volcanic action forms, Dr. von Tschermak conjectures, a stage or phase of igneous concentration, through which all celestial

* "Uber den Vulkanismus als Kosmische Erscheinung," by G. von Tschermak; *Sitzungsbericht der K. k. Akademie, Wien*, vol. lxxv. (March 8, 1877).

† These *Notices*, vol. xxxvi., p. 241.

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bodies, large and small, pass in cooling down, before they are finally consolidated into quiescent planets in which volcanic agency has become extinct.

Among the descriptions of the largest kinds of meteors recently observed, a very valuable notice of that which visited the district of India at the confluence of the Chenab, the Sutlej, and the Indus, towards sunrise on the morning of the 23rd of September 1873, was communicated by Major G. Noel Money, F.R.G.S.,* to the British Association in August last, as seen at Bhawnepoor, near Bawalpoor and Kheirpoor, on the east or left bank of the Sutlej, where it appears from his description that far the largest masses of the stones fell. The light, the roaring sound, and double explosion heard at Bawnepoor were prodigious, and it was ascertained later in the day that a shower of stones had fallen 18 miles north east of Bawnepoor (near Kheirpoor), whence some pieces were brought in. "The largest was an irregular mass, as far as I can recollect about three feet long and a foot thick, still hot, blackened outside as if by the action of fire, of which it smelt strongly. . . . The natives who brought these in said there were many more; one they declared was 'as large as a bullock cart,' and so hot that they could not touch it." Major Noel Money adds that another shower, at the same time, fell at a place 30 miles distant from the first, which was apparently the other extremity of the oval area, about 3 miles wide and 16 miles long, over which it is known by Major Minchin's report on the stonefall at Bawalpoor† that the shower extended. The direction of the axis of this area is about 35° E. from S.; and sufficiently exact descriptions of the meteor's course are given in Major Minchin's report to determine approximately this aërolitic fireball's radiant-point. Uncorrected for zenithal attraction, it must have lain within about 5° of R.A. from a line joining the points 325°+30° and 335°+15°; and with a rough allowance for the effect of the Earth's attraction on a slow-moving meteor, travelling almost horizontally from west to east, the true radiant-point of its flight cannot have lain far (within 6° or 8°) from a point at 320°+20°, a region between *Delphinus* and *Pegasus*, from which a notable number of shooting stars are directed in September. The fireball was one of the grandest seen for many years, resembling in general appearance that of the 21st of December, 1876, above described, with the addition that the vast cluster of fireballs which sped with it across the sky left a broad wake of light-clouds visible on its track for upwards of an hour, extremely luminous, until lost in the brightness of the Sun's rising rays.

One of the rarely grand and magnificent examples of a meteoric fireball was also presented, during the past year, in

* See *Nature*, vol. xvi., p. 375 (August 30, 1877).

† See the *British Association Reports*, vol. for 1875, p. 237, and vol. for 1874, p. 300.

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South Africa by a detonating meteor seen in Cape Colony, at about 8 P.M. on March 17, 1877. The inhabitants of Uitenhage (near Algoa Bay, Bathurst, and Graham's Town, in the south-eastern part of the colony) were thrown into consternation by the intense brilliancy of its illumination, which turned night into day, dazzling the eyes for a space of time described as "nearly a minute." A low rumbling sound like distant thunder followed, after the passage of the meteor from the eastern to near the western horizon, where its bluish-oblong nucleus, nearly as large as the full moon, ended its slow and splendid progress across the sky with an explosion or outburst of sparks in streams of fire. The bush country west of Uitenhage is wild and mountainous, and little expectation can be entertained of aërolites fallen from the meteor in that direction having been recovered. The constellation Virgo was just risen above the eastern horizon at the time of the appearance, and it is in this constellation, a conspicuous region of ecliptic radiant-points in March and April, that the meteor's radiant-point accordingly appears to have been situated. A detonating meteor of large size and brightness is said by the Rev. T. Brockway (who describes its appearance in the *Chronicle of the London Missionary Society*) to have been visible at Fianarantsoa, in Madagascar, on the evening of May 21, 1877, at about 6 o'clock P.M.

The positions of the radiant-points of some recent detonating meteors have been very exactly ascertained. Detonating fireballs made their appearance in Bohemia, and in the northern part of Hungary, on the 10th of April, 1874, and the 9th of April, 1876, a full discussion of whose real paths was made by Professor von Niessl, of Brünn, in Moravia,* with the result that both proceeded from a radiant point near ϵ Cassiopeiæ, the exact place of which (within 1° of R.A. in the two separate determinations) was at $18° + 57°$. This position coincides with no well-determined radiant-point of shooting stars on this pretty-well-established fireball date; and it is also impossible, from the uncertain and defective knowledge of the times of flight and actual velocities of meteors, generally obtained from observations, to deduce correctly the elements of their real orbits round the Sun; yet it is very obvious that the singularly close coincidence of the two radiant-point positions, thus determined, indicates a real community of these meteors in their orbits, denoting their derivation from a common meteor stream similar to those which produce the appearances of meteor showers. The observed velocity of the fireballs Professor von Niessl found to exceed somewhat the theoretical velocity for a parabolic orbit; but similar results are of such very common occurrence, even in the best observations of well-known shower-meteors, that full confidence in the conclusion to which, in common with Professor Galle, Professor von

* "Verhandlungen des Naturforschenden Vereines in Brünn," vol. xiii. p. 81; and "Beiträge zur Kosmischen Theorie der Meteoriten," *Sitzungsberichte der K. k. Akad. der Wissensch. Wien*, vol. lxxv. pt. 2 (April 19, 1877).

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Niessl has been led from this investigation that aërolitic fireballs differ from ordinary meteor showers by moving in hyperbolic, instead of in nearly parabolic orbits, must for the present be reserved until more certain proofs of such a distinction, if it exists, may perhaps hereafter be afforded.

In connection with such investigations the example of the detonating fireball, recently observed in England at $8^h\ 24^m$ P.M. on November 23, 1877, may here be noticed as furnishing some useful observations. The meteor began its course about 96 miles over the centre of Derbyshire, and, with a course of 135 miles, inclined about 40° to the horizon, towards the W.N.W., crossed the coast of Cheshire, nearly over Liverpool, and burst about 14 English miles above the sea, 20 miles north from the opening of the Menai Straits. The radiant-point, and other particulars of this fireball's flight, have been carefully determined by a comparison of very numerous observations, affording very conclusive and satisfactory results, by Captain Tupman.* A loud double report followed the meteor's disappearance both on the mainland and in the Isle of Man. The position of the radiant-point is given very exactly, by five fully described lines of flight, at 62°+21°. In apparently very close connection with the real course of this great fireball may be cited many observations of a detonating meteor which appeared in England at $6^h\ 8^m$ P.M. on November 21, 1865.† This large meteor's visible course was well observed by Mr. F. C. Penrose at Wimbledon, and its disappearance there was followed, at an interval of time corresponding exactly with the distance from his point of view to the end point of the meteor's course, by a loud explosion resembling the distant discharge of a cannon; the sound was not noticed elsewhere, and the general brightness of the meteor was not so great as to lead it to be very widely and generally observed. Its course was, however, well noted by the stars at Oundle, near Nottingham; and many accounts of its appearance at other places were collected by Mr. Warren De la Rue, who observed it at Cranford, and who supplied the Luminous Meteor Committee of the British Association with the materials of many distant observations communicated to him, for the purpose of determining its real course. The radiant-point obtained from two astronomical descriptions of its path was at 56°+13°, a position near which was adopted in the calculations as being in satisfactory agreement with all the more general descriptions, and a probable identity of this radiant point (near λ *Tauri*) may now be

* See *Nature*, vol. xvii., p. 246, for the general results, and *The Observatory*, vol. i., pp. 316–322 (*et seq.*), for the particular details of this discussion. With an average duration of about eight seconds (from twenty-three estimations), the observed velocity of the meteor was found to be 17½ miles per second, while the parabolic velocity, with the observed radiant-point, is 19 miles per second.

† *British Association Reports*, vol. for 1866, pp. 66–70, and p. 125.

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very certainly assumed with that of the great detonating meteor of the 23rd of November last. The course of the detonating meteor of November 21, 1865, was from 41 miles above the Nore (at the mouth of the Thames) to 27 miles above the town of Henley-on-Thames, and from the mean recorded durations of its time of flight, the observed velocity of its motion was only 11·5 miles per second, the parabolic speed for this radiant-point being at the same time 19 miles per second, very much exceeding what was actually observed.

It is remarkable that detonating meteors of which the real paths could be approximately determined had already previously been observed in England on the nights of November 19, 1861; November 27, 1862; and November 20, 1864, * of which it must yet be admitted that only the first can possibly have been a member of the same meteor system as that from which the two more recently recorded fireballs seem to have proceeded. The fireball of November 20, 1864, perhaps detonated (from a description of it in Rutlandshire), but its recorded paths among the stars shows it to have had a northern radiant-point at about 190°+80° (Head of *Camelopardus*), distinguishing it entirely from the *Taurid* meteor group. † Courses of the two previous meteors, however, astronomically recorded by Mr. T. Lynn at the Royal Observatory, Greenwich, and by Mr. Lowe at Grantham respectively, while proceeding each from the direction of the above *Taurus* radiant-point in two opposite directions along the ecliptic, requiring for complete reductions to be combined with other simultaneous observations of the two meteors to which they respectively belonged, proved, when so compared with a multitude of descriptions of those very striking aërolitic meteors, to be really conformable to points near the ecliptic, too remote from the above place in *Taurus* to be strictly or nearly identifiable with it. According to the combined reductions, the first meteor passed from over Paris to a point of explosion and of very loud detonation between Ipswich and Norwich; the second meteor appeared over the mouth of the Scheldt, and disappeared over that of the Seine, between Caen and Havre, with a very loud explosion. If the best available English descriptions of their courses are applied to obtain their radiant-points, ‡ that of the former meteor had a position in *Cetus* (near α), at 45°+5°; and that of the second was at 100°+30° near θ *Geminorum*. Several distinct radiant-points exist, from one observed by Captain Tupman on October 14, 1869,

* *Ibid.*, vol. for 1862, pp. 26–30 and 79; vol. for 1863, pp. 230–240 and 324; vol. for 1865, pp. 80 and 121.

† No resemblance of this radiant-point with that of the detonating fireball seen in America on the 20th of November last (*see* the annexed list of real courses of large meteors) can be suspected; although that meteor also descended from a point having high declination in the northern part of the sky.

‡ It is to be regretted that no exact observations of these two fine meteors, which would have precisely defined their real paths by the most favourable possible combinations with the English observations, were furnished by the towns and observatories of France and Belgium, over whose neighbourhood they passed.

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at *a Ceti*, to that of the *Geminids* of December 12, near *θ* and *τ* *Geminorum*, at 105°+30°, situated in that part of the ecliptic which is near the constellation *Taurus*, during the month of November and in some parts of each of the adjacent months, some of which, it appears, produce detonating, or aërolitic meteors, as well as fireballs and shooting stars of ordinary brightness. Several bright meteors were seen to radiate very accurately from *α Ceti* by Mr. E. F. Sawyer, in Boston, U.S., on the nights of October 17, 20, and 21, 1876,* which is nearly in accordance with Capt. Tupman's shower. Observing the skies nightly in that month and next, Messrs. Corder and Denning in England saw the first members of the well-known shower of *Taurids* of November, with a radiant-point at 61°+18°, commencing on the 21st of October. On the 8th and 20th of November (A.M.), 1876, (and on many intermediate days of the month), the same radiant-point was very conspicuous, nine meteors of great brightness observed by Mr. Denning on the latter day exhibiting it very sharply at 62°+22°. A similar observation to this last was already previously recorded by Mr. Wood in 1861, who noted at Weston-super-Mare for several evenings about November 24 (when the largest and brightest, equal to *Sirius*, was seen) many meteors diverging from a locality between the *Pleiades* and *α Ceti*.† The meteors of this shower, whose activity in November has not before been shown to be so long-continued, but which has certainly for many years contributed a large proportion of the most conspicuous meteors in November, are now designated "Taurids I.," to distinguish them from a closely adjoining shower, "Taurids II.," from between *ζ* and *β Tauri*, detected by Mr. Denning (and also seen by Mr. Corder) in November and December of that year, with a well-marked maximum on December 6 (A.M.) at 80°+23°. Another ecliptic star shower (the "Gemellids") radiates from near *δ* *Geminorum* (107°+23°) in November, and was found very active by Mr. Denning and Mr. Corder between October 25 and 29, and between November 8 and 17, its radiant-point in the latter period being observed by Mr. Denning at 110°+22; and finally, on the 11th and 12th of December, the *Geminids*, a pronounced annual shower with a radiant-point between *τ* and *θ* *Geminorum*, at 105°+32°, makes its appearance, the first member of which in 1876 was noted by Mr. Denning on November 21. Among the known cometary orbits a resemblance to the star shower "Taurids I." is presented by the zodiacal 2nd comet of 1702, an appulse of whose orbit to the Earth's (six million miles below it) occurs on November 27 with a radiant-point at 56°+20°. To this position the detonating (?) meteor of November 21, 1865, the star shower of November 20, 1876, and the detonating fireball of November 23, 1877, are all so nearly conformable that, taking into account the small

* *Astronomical Register*, vol. for 1877, p. 124 (May 1877).
† *British Association Reports*, vol. for 1862, p. 30.

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inclination of the comet's orbit, and that it was not at this, but at another approach to the Earth's orbit in April, that the comet was seen, and roughly observed very near the Earth, an identity of the orbit with that of a meteoric stream crossing the Earth's path about November 20–23 may perhaps be conjectured from the general agreement there of the apparent radiant-points. But, from the slenderly supported character of the observations, no such certainty yet exists in the resemblance as would be required in a comparison between cometary and meteoric orbits to make it a safe and satisfactory basis for pronouncing positively and decisively on the question of their possible physical connection. Whether the two earlier aerolitic meteors of November 19, 1861, and November 27, 1862, were members of the same special meteor-system rather than of some one of the concomitant ecliptic meteor showers in November, which have just been described, appears to be much more doubtful and uncertain, and further observations will be required to distinguish apart, and to determine the exact durations of these several very nearly ecliptic November showers.

Good observations of fireballs seen on January 19, March 17, April 6, April 16, June 14, and October 19, 1877, have been obtained, making it possible to determine the localities, and the real heights and directions of their courses with more or less exactness, only amounting in one case, however (that of the fireball of March 17), to the certainty and precision with which that of the fireball of November 23, 1877, was ascertained. The calculation of that meteor's real path is also by Captain Tupman,* and M. Gruey computed that of the fireball of June 14, 1877. The approximate heights and real courses of the other meteors in the list were extracted from various simultaneous observations of each meteor (for the most part not very numerous or exact), received by the Luminous Meteor Committee of the British Association, by Professor Herschel.

* *The Obervatory*, vol. i., p. 19 (April 1877). M. Gruey's calculation appears in the *Comptes Rendus*, vol. lxxxv., p. 632, October 1, 1877. The real path of the fireball of July 8, 1876, described in the list, is extracted from Professor Kirkwood's Paper (already quoted) "On eight meteoric fireballs, &c."; and the occurrence of the fireball was noticed in this series of Reports, last year; these *Notices*, vol. xxxvii., p. 211.

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Results of Double Observations of some Large Fireballs Seen in the Years 1876 and 1877, showing their Real Paths and Radiant-points.

Date and Hour (Local Time, or) G.M.T. Size and General Appearance.	Principal Places of Observation.	Meteor's Real Course. Distances in British Statute Miles. Beginning: Height and Locality.	End: Height and Locality.	Length of Path, and Velocity.	Observed Radiant-point.	Nearest known Radiant-point, and Remarks.
1876, July 8 ($8^h\ 45^m$ p.m.) Glare of its light at Chicago stronger than that of full moon. No sound, or terminal explosion, but a luminous streak left visible for 40 minutes.	Bloomington, Paoli, and elsewhere, In.; and at Chicago.	88 miles above Ottohee, Ohio, near western point of Lake Erie.	34 miles above Lake Michigan, 20 or 30 miles from Michigan and Chicago.	145 miles. No estimates were recorded of its time of flight. (Calculations of the height and position of its real path by Prof. Kirkwood.)	About 305°+7°; near *k* (Bode) *Aquilæ*.	305°+9°, June 28, Tupman No. 35. 305°+5°, July, Neumayer. *Aquilids* II (Greg.). A numerously attested shower.
1877, Jan. 19, $6^h\ 27^m$ p.m. ½ diam. of Moon, at Bray, when disappearing, with sparks. Nucleus white or bluish, with brilliant train; left no streak.	Bray, and Lisburn, Ireland; Walsall and Wolverhampton, England.	75 miles over St. Ann's Head, Milford Haven.	45 miles over the mouth of St. George's Channel, 130 miles S. of Cape Clear.	230 miles in 6½ secs. (three accounts between 5 secs. and 8 secs.). 35 miles per sec. (parabolic speed 23 miles per sec.).	135°+27° (±6°), near *ν Cancri*.	130°+20°, Dec. 21.–Jan. 5, 1877, Denning. A feeble shower, confirmed in December by Schmidt, and by Schiaparelli on Feb. 13.
1877, March 17, $9^h\ 57^m$ p.m. ¼ or ½ diam. of Moon; brilliant in Western England and Ireland. Nucleus blue, with fiery sparks on track.	Cambridge and Frome, confirmed at Tetbury and at Gunnersbury (London). Seen at Brighton, Fleetwood, Waterford, &c.	60 miles over Taunton, Somersetshire.	29 miles over Pontypool, Monmouthshire.	58 miles in 3 secs., Frome. Other estimates 2, 3, 3, 4½ secs. About 19 miles per sec. (parabolic speed 13·5 miles per sec.).	145°–4° (fairly accurate). In *Sextans*, near *Cor Hydræ*.	No previously known radiant-point in this position. (Calculations of the meteor's real course by Capt. Tupman.)
1877, April 6, $9^h\ 26^m$ p.m. Light equal to (at St. Ann's Head), and stronger than that of full Moon in the South of Ireland. Globular; light blue, with crimson wake and slender yellow tail; flashed once or twice, and burst with sparks and loud detonation; left no streak.	Shillelagh and Thomastown (Kilkenny), Dublin, Drogheda, and Roche's Point, Cork Harbour; and St. Ann's Head, Milford Haven.	80 miles over Portarlington (Queen's County), or Kildare.	20 miles over Cape Clear.	165 miles in 5½ secs. (three estimates between 3 or 4 and 7 or 8 secs.). 31 miles per sec. (parabolic speed 24·5 miles per sec.).	275°+50° (±5°), near *γ Draconis*.	267°+53°, March–April 23, Greg. No. 47, DG, *Draconids* I. 262°+47°, April 1, No. 46, 263°+50°, ,, 14, ,, 57, Schiaparelli.

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1877, April 16, $10^h\ 50^m$ p.m. ♂ or 3× ♀ (Leicester). Cast a strong glare, bursting twice; slightly zigzag, rocket-like, blue; tail yellowish; disappeared suddenly; no streak.	Cambridge, Leicester, London, and Bristol; and Douglas, Isle of Man.	60 miles over southern part of Yorkshire (locality uncertain).	30 miles over the North Sea, near the coast of Yorkshire.	30 or 40 miles in "a second or two." (Length and exact position of real path uncertain. Parabolic velocity 11 miles per sec.).	140°+50°; θ *Ursæ Majoris* (about 20° W. of the Zenith; position only roughly indicated).	147°+47°, Mar. 20–Apr. 23, No. 45 (MZ), Greg. A well-marked centre; Heis, Schiaparelli, Denning.
1877, June 14 ($8^h\ 5^m$ p.m.) Diam. 5′ or 6′, attaining at last the size and brightness of full Moon. White, with considerable tail of bright red and blue tail. Detonation heard in 5 minutes, at Bordeaux.	Clermont Ferrand, Angoulême, and Bordeaux, France.	157 miles (?) over a point about 80 miles S.W. of Nerac, Gers.	27 miles over a point about 10 miles W. of Bibarac, Dordogne. (55 miles distant from Bordeaux.)	170 miles (?) in 4 seconds (mean of three estimations); velocity 42½ miles (?) per second (parabolic speed 11 miles per second).	212°+12° (±3°), near ζ *Boötis*. (Calculations of the real path by M. Gruey.	202°+9°, May, Heis. 210°+20°, July 4–11. Q., Greg. No. 89. No known position near to it in June.
1877, October 19, $6^h\ 13^m$ p.m. Large, white, "balloon-like"; no explosion. Left a white light-band, bright and becoming serpentine, for 8 or 10 minutes.	Monmouth, Swansea, Bath, Weston-super-Mare; Dublin, and near Waterford and Templemore, in Ireland.	60 miles over Milford Haven.	40 miles over Cape Clear.	(With the exception of Monmouth, and Dublin, no particulars of position suitable for exact combination together were recorded at any of the points of observation.)	20°+15° (?), near η *Piscium* (accords with the Monmouth track, if the meteor's path, from east to west, was nearly *horizontal*?)	20°+14°, near η *Piscium*, Oct. 13–29, 1876, Denning. An active radiant-point of bright slow-moving meteors. (No dependence can be placed upon the *assumed* observed position of the radiant-point.)
1877, November 20 (aftn.) Splendid fireball; flaming tail and long-enduring cloud-streak. Terrific explosion over Danville, Halifax, &c., N. Ca.	Richmond, Washington, Bristol, Halifax, and other places in Virginia and N. Carolina, U.S.A.	70 miles above a point 15 miles N. of Danville, Virg.	10 miles above a point 25 miles, a little SE. of, from Danville, Virg.	About 70 miles (by description of the real height and position).	About alt. 60°, 35° N. from W. (by description of the real course).	Real course of the meteor in a letter by Prof. H. A. Newton, in the *Richmond* (U.S.) *Daily Dispatch*, Dec. 13.
1877, November 23, $8^h\ 25^m$ p.m. Great detonating meteor; ½ diameter of Moon; of extreme brilliancy; streak 40 miles long, and 2,000 feet in diameter. Explosion heard in Isle of Man, N. Wales, and Cheshire.	England, Wales, Scotland, and Ireland.	95 miles above a point 13 miles N. of Derby.	14 miles above a point 19 miles NNW. of Llandudno.	135 miles. Velocity, from 25 estimations of the duration, 17½ miles per sec. (Parabolic speed 19 miles per sec.)	62°+21° (±3°); near ε *Tauri*.	A slight alteration of the radiant (diminishing its longitude) brings the orbit nearly into coincidence with that of the comet of 1702. A well-known radiant, *Taurids* I.

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The few accordances with known radiant-points of shooting stars, comets, or of previously observed fireballs are noted in the last column of the table. The fireball of June 14, 1877, is quite unconformable to the course of that of June 17, 1873, as found by Professor Galle* and Von Niessl in their recent discussions of its track. With accounts of that meteor's appearance collected nearer to the end-point of its flight over Moravia and Bohemia than most of the observations of it used by Professor Galle, the same position of its real course in every particular of direction, length and velocity, and point of disappearance, as that found by Dr. Galle, is found by Von Niessl to be perfectly established with scarcely any sensible departures.† In the beginning of its flight, especially, a new observation at Schemnitz, in Hungary, entirely confirms the great length of path assigned to it by Dr. Galle, fully two thirds of which were very brightly traversed before the light-clouds (the only part of its track at first believed by Professor Von Niessl to have been visible), were deposited which marked about the last third part of its course with an enduring light-band shining brilliantly in the twilight sky for half an hour. Like Dr. Galle, Professor Von Niessl assigns to the meteor a hyperbolic orbit round the Sun, but even with the additional descriptions obtained in his latest enquiries and rediscussion of the meteor's course, the meteor's real speed first found by Professor Von Niessl, of $19\frac{3}{4}$ English miles per second, remains unaffected, and is much less than that of $28\frac{1}{4}$ miles per second found by Dr. Galle. The theoretical velocity of a meteor with the same radiant-point, for a parabolic orbit, is about $15\frac{1}{4}$ miles per second. The correction of the apparent radiant-point for zenithal attraction when a slow-moving meteor's path is nearly horizontal, amounts to several degrees (about 5° in the present instance), and depending as it does on the unknown element of the meteor's velocity, the exact estimation of its amount is uncertain; and although the meteor's apparent radiant-point at 248°·6, −20°·2, and at 246°·7, −19°·3, is almost indifferently assigned at the same place by both the calculators, yet it is so nearly on the ecliptic that the doubtful effect of the Earth's attraction upon its resulting place makes it uncertain whether the meteor was in the ascending, or in the descending, node of its orbit at the point where it overtook the earth, and the example which here presents itself is again that of a detonating meteor with its orbit lying very nearly in the plane of the ecliptic. Whether it had any natural connection with that of the fireball of July 25, 1876, with a radiant-point near *Antares*, also very near the ecliptic at

* See the Reports for 1875, in these *Notices*; vol. xxxv., p. 247.

† "On the Meteor of June 17, 1873," by G. von Niessl, Brünn, 1874. Excerpt (of 55 pages) from the *Verhandlungen des Naturforschenden Vereines in Brünn*, vol. xii. (See also the *British Association Reports*, vol. for 1877, where the author's remarks and conclusions in the Memoir are reviewed.)

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258°—24°,* will depend upon the possible extension of meteor streams in perihelion distance, which may result from the effects of planetary disturbances upon their course. The orbit of the second comet of A.D. 568, which distantly agrees with that of the later fireball, at its ascending node, appears to have had a sensible inclination to the ecliptic, and accordingly not to be so easily reconciled with a common stream to which these two fireballs may possibly have belonged.

The bolides of January 19, April 6, and October 19, 1877, all passed from east to west over or near the south coast of Ireland, with long and very brilliant courses, that of April 6th bursting with a loud explosion about 50 miles from Cork Harbour, and that of October 19 leaving a light band in the sky which assumed curiously distorted figures before it disappeared. The great luminosity of the streak before its dispersion must have been owing in great measure to its being brightly shone upon by the Sun's rays, which had but shortly set, at the great height at which it was deposited, and which tended to make it a very brilliant spectacle in the western counties of England, even in the strong western twilight, where it presented itself, while at Templemore, in Ireland, the sky seemed to be cleft asunder by the long track which the meteor left behind it. It is to be regretted that none of the views of the meteor obtained in Ireland supplied materials of its apparent path sufficiently exact by combination with those in England, for determining its real course.

Bright meteors were seen on February 4 and 26; March 11, at St. Etienne in France; June 4, July 2, August 16, September 7 (in the United States), 11 (at Boën, Loire, in France), and 12 (at Geneva), on October 8 and 29, and on December 2, 1877; and *Venus*-like meteors were noticed on March 28, October 2, 3, and 8,† November 4, 16, and December 8, 1877, some accounts of which that have reached the Luminous Meteor Committee of the British Association are yet insufficient to localise their real courses with exactness. The meteor of June 4 was stationary, 2° above ε *Pegasi* at 323°+11°, or about three seconds "as if the sky had opened and shown the moon through, and then closed again," as seen by Mr. J. K. Esdaile at East Grinstead (Sussex). That seen by Mr. R. Copeland at Lord Lindsay's Observatory, Dunecht (Scotland), with a flash bright enough to be noticed within doors, at about 8 o'clock P.M. on October 29, 1877, must also have been directed nearly towards the observer, for it left a short bright crooked train scarcely ½° long, at 268°+60°, directed towards ξ *Draconis*. In the *Scientific American* of September last, Professor

* See the Report for 1877, in these *Notices*, vol. xxxvii., p. 208; and the *Reports of the British Association*, 1876 and 1877, for particular descriptions of the meteor.

† At midnight (Bristol), very near the east horizon (109°+17° to 116°+12°). One or two bright meteors on this evening were also seen in France (*Bulletins de l'Association Scientifique de France*, vol. xxi., p. 224).

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D. Kirkwood wrote that a few minutes after 10 o'clock on the evening of September 7, Mr. John Graham, at Bloomington, Ia., attracted by a sudden light, saw a stationary meteor between *Aquila* and *Vulpecula et Anser*, at 295°+15°, moving scarcely ½° for a second or more, while it was visible. Within a few seconds afterwards three meteors of about the brightness of first magnitude stars appeared and disappeared at the same place successively, with motions of not greater extent than the first had, in different directions; and watching a few minutes more, a fifth nearly stationary meteor made its appearance there, of about the same brightness as the three previous ones. The date and position of this shower are new, but they do not appear to be very far distant from an already pretty well known radiant-point (Tupman, 55, August 23, 298°+28° and 302°+21°). Mr. Denning states that, besides the *Venus*-like meteor on October 2 (at 9^h P.M.), which left a light-streak when it burst for three and a half minutes, he also noted at 9^h 46^m and 11^h 17^m meteors as bright as *Jupiter* and *Sirius*, and several other meteors between 7^h 30^m and 12^h 30^m on that night radiating very distinctly from a point at 225°+52°. A meteor as bright as *Venus* at 8^h 38^m P.M. on October 3 diverged with several others from a radiant-point half-way between *Polaris* and *α Ursæ Majoris* (165°+77°), and among more than 100 meteors noted on the nights of October 2–4, 1877, thirteen proceeded from a very well defined radiant-point at 130°+79°, which appears to agree with a cometary radiant-point (that of Comet II., 1825) at 134°+77°, for October 7. The bolide seen by Mr. Wood at 8^h 4^m P.M. on November 4 proceeded from a radiant-point in *Musca*, at 44°+24°, of meteors seen plentifully by Mr. Denning on the nights of October 31 and November 1, coinciding in place with an active shower (for September and October) which he traced continuously, in 1876, until November 17. That observed by Mr. Corder on December 8, at 6^h 23^m, is directed from the same point, but by the slowness of its motion may have been from a nearer radiant-point between *Taurus* and *Cetus*; it was pale green—a rare colour in meteors—as was also one *Geminid*, as bright as *Sirius*, seen on December 9, and another equal to a first magnitude star seen on December 10.

A vast multitude of meteors has during the last eighteen months been observed and mapped by Mr. Corder and Mr. Denning, and almost numberless tracks, contained chiefly in foreign catalogues, have at the same time been reduced to their radiant-points by Mr. Denning. The number thus observed must amount to about 3,000, and of those examined and in great part mapped and (excluding well-known special showers) reduced to meteor centres of good positions and of no great durations, to 10,000 or 12,000. Between thirty and forty meteor streams, new in their directions or durations (and, many of them, especially, in their directions), have thus been contributed, often by morning watches of the sky, to the already known complement of ordinary star-showers; and about 100 of the known list of 200 or 220

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Date (and Duration) of *Meteor* Shower; and Remarks.	Position of Radiant Point. α δ	By the Stars.	Comet Shower. Position of Radiant. α δ	Day of Shower.	Comet; and its Node, or Appulse.
	h °		h °		
1877 (July 6–20, 6 meteors;* not *Perseïds*)	36 + 47	*θ Persei*	39 + 45	July 8	770, ♂
1877 (July 12–20, 5 meteors;* do.)	47 + 45	*κ Persei*	49 + 45·5	July 25	1764, ♂
1877, Oct. 16; meteors very plentiful; this the chief shower	133 + 20	*δ Cancri*			
1876, Oct. 15 (from Sept. 20—Nov. 17)	46 + 26	*Musca* (47 + 29, Oct. 8, 1877)			
1877, Oct. 31 and Nov. 1 (13 meteors)	42 + 22				
1876, Oct. 30 (from Oct. 25 to Nov. 23)	110 + 23	*δ Geminorum*	104 + 27	Nov. 4	837, I., ♂
1876, Nov. 8 and 20 (a.m.), (from Oct. 21—Nov. 20)	60 + 20	*ω* (between *ε* and *η*) *Tauri*	56 + 20	Nov. 27	1702, II., ♃
Brightest on Nov. 20 (a.m.)	62 + 22				
1876, Nov. 26 (from Nov. 21—Dec. 20)	208 + 43	Near *η Ursæ Majoris*	(205 + 48	Nov. 1	1097, ☉ ?)
1876, Nov. 26–29, max., 21 meteors; (Nov. 20—Dec. 13)	155 + 36	*Leo Minor*	162 + 34	Dec. 2	1798, II., ♂
1876, Dec. 6 (Nov. 20–Dec. 8)	81 + 22	*β, ζ Tauri*			
1877, Jan. 4 (Jan.)	57 − 12	*γ Eridani*			
1877, Jan. 17; well-defined shower (Jan. 5–20)	295 + 53	*ι, ψ Cygni*			

* Slight showers of streak-leaving meteors, distinct from the true *Perseïds* first seen on July 29.

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ordinary meteor showers have at the same time received more or less frequent and ample confirmations. Some of the new observations were of bright and abundant, though fugitive, showers, with very well marked dates of maxima and centres of divergence; a short list of these is annexed (principally seen before January last, no shower of very well marked dates, although many diffuse ones with frequent, pretty accurately diverging meteors, having been observed since then). A complete list of all the observations and reductions was compiled by Mr. Greg, and was presented in the last report of the "Luminous Meteor Committee" to the British Association, together with a list of the cometary radiant-points with which they appeared to stand in some connection. Of these, some are included in the annexed list, and a complete catalogue of all such correspondences (now 70 in number) is intended shortly to be presented to this Society by Professor Herschel, in which the best instances of resemblance between cometary and meteor shower orbits detected among these observations will be found recorded. Of those which are entirely new there are six examples, and ten which better establish already known similitudes to comet orbits than any previously produced among the thirty-five cases of apparent coincidence with comet showers which the new observations have been found to present when compared with those comets to whose orbits the Earth approaches most nearly in its annual path.

The loss which contemporary astronomy sustained last year in one of its best observers and most distinguished and honoured representatives, Professor Heis, has fallen especially upon that branch of its study (open and attractive to all) which he pursued with great distinction. Meteors, auroras, the constellations and the milky way, comets, and lastly, the zodiacal light, all of them objects which can be studied with the unassisted eye, received the principal share of his attention, and presented themselves in new aspects from his skill in recording and representing them. One of the first to recognise the importance of referring shooting stars to their radiant-points, he introduced method and system in their observation, and a regular nomenclature of meteor showers, after many years of observations and reductions, in the year 1849. These first essays of collecting observations he continued in succeeding years by superintending simultaneous watches for shooting stars at the Münster, and at other neighbouring Observatories and stations in Germany, until forty-three years from the beginning of his undertaking, in the year 1833, had elapsed. Since the end of the year 1875 Professor Heis collected all the records and reductions of this long period, with their full discussions, in a single volume, which appeared as he intended, although not until a few months after his untimely death, in the publications of the Münster Observatory (1877, part ii.) as a closely printed 4to. pamphlet of 180 pages. It contains the recorded paths of about 13,000 meteors observed at Aix-la-Chapelle (before the year 1852), and at Münster, and

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about 3,000 meteor-tracks recorded elsewhere, often corresponding to those seen at Münster. Besides constant references to contemporary observations, many of them reported at much length in Heis' *Wochenschrift für Astronomie*, a discussion of the corresponding observations, in the narrative portion of the work, gives the heights at first appearance of 246, and at disappearance of 273 meteors (with the velocities of six meteors), presented in a table so as to show the relative frequency of the observed heights at appearance and disappearance, at different distances from the Earth. The radiant-points of the corresponding tracks are included in a catalogue of meteor showers which concludes the book (sixteen of which appear to be founded upon them), wherein all the meteor-tracks of the general list are enumerated which are conformable to the several centres of divergence. The observed places of 36 "stationary" meteors also furnish as many new radiant positions in this list, which embraces 230 originally observed radiant-point positions. Nineteen meteor-apices of Mr. Greg's catalogue and 85 of those obtained by Schiaparelli from Zezioli's observations are added to the number, as established and corroborated by some of the meteor-tracks recorded in the general table. The number of Professor Heis' originally determined meteor showers is thus greatly increased from the preliminary list which he communicated in the year 1864 to this Society (these *Notices*, vol. xxiv., p. 212), and which, revised and extended in the year 1867 (*Astronomische Nachrichten*, No. 1642), embraced 83 showers for the whole year. But the present additions are principally in July, August, and the autumn months, and only slight extensions of the older lists in the first six months of the year, from January to June, occur in the present catalogue. Dates and periods of considerable frequency of shooting stars are noticed; and remarks on unusual appearances of brightness, of curved paths, and of other peculiarities of individual meteors were partly described, and were apparently intended to be more fully collected and extracted from the records had the concluding appendices of the work not been prematurely interrupted, and the final publication of its carefully digested pages left unexpectedly to the care of surviving relatives, and to able pupils of its distinguished author, by his lamented death.

Among the special major star showers of the past year few have been very conspicuous. The *Geminids* of December 11–12, 1876, formed rather an exceptional return of that meteor shower, its first representatives being seen by Mr. Denning on November 21, and by Mr. Corder on December 4, and its time of maximum occurring apparently about midnight on December 11. Two of M. Tisserand's assistants, at the Observatory of Toulouse, counted at that time 106 meteors in two hours, with a radiant-point (according to M. Perrotin) at 115°+33°, about 10° east of its place (105°+32° *r Geminorum*) in the general catalogue of Mr. Greg, who first found its position, together with Professor

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Twining, in America, in the year 1862. Mr. Corder's and Mr. Denning's observations gave, respectively, the places 107°+35°·5 and 106°+32° for its position. Mr. Backhouse also observed them at Sunderland, and their rate of frequency in England on the night of the 11th was from 20 to 24 meteors per hour for one observer. Two as bright as *Venus* and several as bright as *Jupiter* were noted by Mr. Denning.

No return of the *Lyrids* in April 1877 was recorded in England, this shower (and also the *Coronids*, a well-marked April shower) being represented by only one or two meteors each during a continuous watch of five hours, with clear sky, kept for them by Mr. Denning on the night of April 19–20; but the shower is known to be so transitory and narrow-zoned in its main stream, that its invisibility in April last must not yet be too hastily inferred from this single observation.

The *Perseïds*, in August 1877, were watched for in England under somewhat unfavourable conditions of cloudy weather; and the display does not seem to have been a rich one. Many meteors of the shower were observed at the Royal Observatory, Greenwich; but few, owing to cloudy nights, were noted at the Radcliffe Observatory, Oxford. In five hours on the night of the 10th of August, 1877, Mr. Denning observed alone, at Bristol, 285 *Perseïds* and 69 unconformable meteors; in the proportion of about 4:1. In 1876 the proportion of conformable to unconformable meteors was about 2·3:1, from August 8–12 inclusive; showing no great superiority in last year's display over the meagre exhibition of the shower in the year 1876. Bursts of 9 meteors in one minute, and 6 meteors in half a minute were observed at $12^h\ 40^m$ and $14^h\ 15^m$ on August 10, and meteors were more numerous and brighter on the 10th than on the 11th of August. Only two or three were seen as bright as *Jupiter* and *Venus*, leaving streaks for 20 or 30 seconds; but of these one was directed from a new radiant-point, near *Perseus*, at 96°+71° (near *p*, *q*, Bode, *Camelopardi*), not a true *Perseïd*. By a close examination of twelve or thirteen thousand meteor-tracks recorded between the 6th and 12th of August in various recent meteor catalogues (chiefly in morning watches, by foreign observers), Mr. Denning has succeeded in singling out seventeen such radiant-points in constellations east of *Perseus* which are more or less active during the *Perseïd* shower, and in reducing 680 of the "unconformable" tracks to these contemporary centres, of which this position, although a new one, is the best defined and the most abundantly marked centre, by 73 meteor-tracks diverging from it very exactly. During the interval from July 6 to August 23, 1877, Mr. Denning observed 390 *Perseïds* and 567 other meteors, and, with the apparent paths of 250 more recorded during September, obtained or confirmed with fresh positions, 89 centres of ordinary meteor showers for the months of July, August, and September.

The *Orionids* were well seen on the nights of October 17–20,

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by Mr. Denning and Mr. Corder, and the following brief abstract of his observations at Bristol represents the good view of them which was obtained by Mr. Denning. Mr. Corder's view near Chelmsford, of 53 *Orionids*, on the mornings of October 17 and 18, corroborates this account, making the hourly number 14 on the 18th, abating sensibly after half-past three o'clock, while a shower of slow-moving meteors from *o Piscium*, in the opposite quarter of the sky, seemed to gain constantly in intensity throughout the night. The radiant-point of the *Orionids* appeared to shift its position slightly from 89°+18° in the morning of the 17th to 95°+17° on that of the 18th of October.

Bristol, 1877 October.	Hours of Watch. A.M.	Length of Watch.	Number seen.		Calculated horary numbers.		State of the Sky.
			All Meteors.	Orionids.	All Meteors.	Orionids.	
	h h	h					
17	1 -3	2	41	13	27	9	Very clear.
18	2 -5	3	70	33	35	16	Slight fog.
19	4¼-5¼	1	19	9	25	12	Fog; stars dim.
20	5 -5½	½	14	2	37	5	Very clear.
17-20	1 -5½	6½	144	57	31	10½	Totals, and average rates.

Maximum October 18, $3^h\ 37^m$—$4^h\ 44^m$ A.M.; calculated number of *Orionids*, 25; horary number of ditto, 22. At $4^h\ 28^m$ A.M. four bright ones appeared almost simultaneously in *Ursa Major*.

Of the 57 *Orionids* 47 left streaks. Three were as bright as *Jupiter*; three equal to first magnitude stars; the rest generally equal to third and fourth magnitude stars. The radiant-point was very accurate, at 92°+15° (57 meteors), with a subradiant strongly suspected at 86°+8°. A much more conspicuous appearance was accordingly presented by the shower in October 1877 than in October 1876.

The *Taurids I.*, which prevailed very abundantly and brightly in November 1876, were a scarcely recognisable shower in November last, only a few stray members of its stream being traced by Mr. Denning and Mr. Corder throughout the month. Several *Leonids* were seen on the mornings of November 11 and 14; as numerous on the latter morning, as observed by Mr. Backhouse at Sunderland, as the conformable shooting stars, or about 6 per hour; some of them bright and leaving streaks. An equal prevalence of the *Andromedes* was also noticed by Mr. Backhouse on the night of November 27, with slighter traces of the same shower perceptible on the evening of the 25th.

Mr. Corder's view of the *Geminids* at Writtle, near Chelmsford, on December 9 and 10, 1877, was very successful, while clouds appear to have veiled the shower at other stations on its annual nights of December 11-12, and only a few additional observations of it could be obtained at Writtle on the 12th. Nearly twenty were noted, chiefly from 9^h to 11^h P.M., on the 9th; and 40 out of

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65 meteors observed between 7^h and 11^h 30^m P.M. on the 10th were *Geminids*, of which 25 were visible in the hour from 10^h 30^m to 11^h 30^m P.M. They were mostly small, only one on that night being equal to a first magnitude star, and very few left streaks; the radiant was also rather diffuse, extending from near *Castor* to *θ Geminorum*, with a mean place at 107°+35°. Contemporaneously with the *Geminids* a shower of bright long-pathed meteors from a closely adjoining radiant-point near *ε Geminorum*, at 108°+28°, was visible on December 9, producing several lesser, streak-leaving meteors, and two fine ones, as bright as *Sirius* and *Jupiter*, remarkable both by their long slender streaks and by the mauve colour of their nuclei. This streak-leaving shower had nearly become effaced on the 10th, and no other meteors belonging to it were afterwards observed on later nights. Mr. Backhouse appears to have recognised it on December 2 and some later evenings, with a radiant-point not far from *ζ Geminorum*.

A brief but bright shower of the *Quadrantids* of January 1–3 was observed on the morning of January 2, 1878, from 4^h to 4^h 30^m A.M., at Hawkhurst, in Kent, by Professor Herschel. Seventeen meteors of this shower, and three unconformable to it, were noted in a nearly clear sky during the half-hour's watch. Two of the *Quadrantids* were as bright as *Sirius* or *Jupiter*, five equal to first, six equal to second, and the rest equal to third or fourth magnitude stars. No streaks were left, but their motion from a nearly vertical radiant-point was short and quick, and that of a very bright one was a little curved. The radiant-point (somewhat imperfectly deduced, from haze which hid many of the smaller stars) was in *Quadrans*, near *θ Boötis*, at 222°+55°. The reappearance of this periodic shower, with somewhat notable intensity this year, appears to corroborate Professor Kirkwood's conjecture of a thirteen-year cycle of this star-shower observable in its principal returns during the present century, in the years 1825, 1838, and 1864, which led him to anticipate a marked recurrence of the shower in January of last year; but no verification of that prediction could, unfortunately, on account of the universally clouded state of the sky at that season, be obtained. A watch for the shower in clear sky near midnight on each of the nights before and after that of its appearance on the morning of January 2 failed to trace any appearance of its meteors in a watch of half-an-hour.

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Papers read before the Society from February 1877 to February 1878.

1877.

Mar. 9. The Katadioptric Aplanate. Prof. Zenger.

Observations of the phenomena of *Jupiter's* Satellites at Adelaide, South Australia. Mr. C. Todd.

On the Visibility of the Unilluminated portion of the disk of *Venus*. Señor A. T. Arcimis.

Observations of Occultations of Stars by the Moon, and phenomena of *Jupiter's* Satellites made at Cadiz in the year 1876. Señor A. T. Arcimis.

Note on the Double Star H. I. 62. Mr. S. W. Burnham.

Note on the relative Space-penetrating Power of Reflectors and Refractors. Dr. T. R. Robinson.

Sur le problème de Kepler. Dr. A. de Gasparis.

On apparent Brightness as an indication of Distance in Stellar Masses. E. J. Stone.

Ephemeris for Physical Observations of *Jupiter*, 1877. Mr. A. Marth.

On the Magnitude of η *Argûs*. Mr. J. Tebbutt.

Corrections to the *Nautical Almanac* places of the Moon, for Longitude Observations in connection with the Transit of *Venus*, 1874. Capt. G. L. Tupman.

On Observations of Contacts of the Limb of *Venus* or *Mercury* with that of the Sun. Prof. S. Newcomb.

On the effect of a Star's rotation on its Spectrum. Capt. Abney.

Note on the Numerical Lunar Theory. The Astronomer-Royal.

On the Nomenclature of Meteor Radiant Points. Mr. T. W. Backhouse.

Total Eclipse of the Moon, February 27, 1877. Rev. S. J. Perry.

Solar Physics. Mr. J. Ennis.

Note on a Transformation of Lagrange's Equations of Motion in General Co-ordinates, which is convenient in Physical Astronomy. Prof. R. S. Ball.

On Photographing Solar Transits by the use of the "Starlit Transit Eye-piece," formerly described, and other methods. Dr. Royston Pigott.

Observations of Occultation of *Regulus*, and of the Total Eclipse of the Moon, February 27, 1877 (with drawing). Mr. F. C. Penrose.

On the Variable Star U *Geminorum*. Mr. G. Knott.

On the Lunar Perturbations arising from the Planet *Jupiter*. Mr. E. Neison.

On two Occultations of Planets by the Moon observed by the Chinese. Mr. J. R. Hind.

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April 13. Some Experiments bearing on the Transit of *Venus*. Rev. S. J. Perry.

The Planet *Vulcan*. Rev. S. J. Perry.

Sur l'application de la Photographie à l'observation du passage de *Venus*. Mons. A. Angot.

Etudes sur les images photographiques obtenues au foyer des Lunettes Astronomiques. Mons. A. Angot.

Report of examination of the Sun's disk at Guildown, Guildford, on March 21, 22, and 23, 1877, for the suspected Planet *Vulcan*. Mr. J. Rand Capron.

Radiant points of Shooting Stars from Capt. Tupman's unreduced Observations, 1869 to 1871. Mr. W. F. Denning.

Visible Transits of *Mercury* to the year 2,000. Rev. S. J. Johnson.

Nebuleuses nouvelles découvertes et observées à l'Observatoire de Marseille. Mons. E. Stephan.

On Personal Equation. Capt. W. M. Campbell.

The great Meteor of 1877, March 17. Capt. G. L. Tupman.

Remarks on the Comets of 1877, with Elements and Ephemeris. Prof. C. Pritchard.

Mechanical Solution of Kepler's problem. Prof. C. Pritchard.

Observations of Occultations of Stars by the Moon, 1875, 6, 7 (with the deduced equations between the error of the Lunar Elements), and phenomena of *Jupiter's* Satellites, made at the Radcliffe Observatory, Oxford. Rev. R. Main.

On an Elliptic Function solution of Kepler's problem. Mr. J. W. L. Glaisher.

On the Inequality in the Moon's Longitude, discovered by Prof. Newcomb. Mr. E. Neison.

Ephemeris for Physical Observations of *Mars*, 1877. Mr. A. Marth.

Extract from a letter describing an Ancient Arabic Celestial Globe. Prof. Meucci.

The Sidereal System. Mr. Maxwell Hall.

Description of an improved Diagram for the Graphical Solution of Spherical Triangles. Mr. F. C. Penrose.

Heliometer Observations of the Planet *Juno*. Lord Lindsay and Mr. Gill,

On the proposed Expedition to observe the approaching Opposition of *Mars*. Mr. D. Gill.

On the Opposition of the Minor Planet *Ariadne* as a means of determining the Solar Parallax. Mr. D. Gill.

Nebuleuses nouvelles découvertes et observées à l'Observatoire de Marseille. Mons. E. Stephan.

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Comet II. 1877 (extract of letter to Mr. Hind). Prof. Winnecke.

May 11. A new Solar eye-piece. Prof. C. V. Zenger.

Lunar Eclipse observed at Cadiz on the 27th February 1877. Señor A. T. Arcimis.

A Table for Facilitating the Determination of Mean Equinoctial Time to the end of the present century. Admiral Sir C. F. A. Shadwell.

On a method of making a Pendulum swing in an approximately Cycloidal Arc. Gen. M. C. Meigs.

Second Catalogue of Micrometrical Measures of Double Stars made at the Temple Observatory, Rugby. Mr. J. M. Wilson and Mr. G. M. Seabroke.

Note on the alleged Fall of an Aerolite at Supino, Italy. Padre Secchi.

Extracts from letters received relating to the Search for the supposed Intra-Mercurial Planet. The Astronomer-Royal.

A Determination of the Semi-diameter of *Venus* at the mean distance of the Sun from the Earth. Mr. A. W. Downing.

On a method of Destroying the Vibrations in a Mercurial Reflector. Mons. De Boë.

The approaching Opposition of *Mars*. Mr. R. A. Proctor.

On a General Method of treating the Lunar Theory. Mr. E. Neison.

Elements of the Orbit of Comet 3 (Swift's), 1876. Prof. C. Pritchard.

On the Opposition of the Minor Planet *Melpomene* as a means of Determining the Solar Parallax. Mr. D. Gill.

On the Spectra of Comets *b* and *c*, 1877. Lord Lindsay.

Note on the Distribution of the Fixed Stars. Mr. R. A. Proctor.

June 8. On Collimating the Axis of Reversible Instruments, such as Equatoreals, Transits, and Altazimuths, by testing for Parallax developed on distant scales. Dr. Royston Pigott.

Occultations observed at Windsor, New South Wales, in the year 1876. Mr. J. Tebbutt.

Note on the Arc of Light seen round *Venus* in transit. Mr. R. A. Proctor.

Note on some of M. Stephan's new Nebulæ. Mr. J. Dreyer.

Description of an Observatory at Higher Bebington, Cheshire. Mr. R. C. Johnson.

Note on the new Inequalities in the Moon's Longitude, pointed out by Mr. Neison. Prof. S. Newcomb.

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On the Collective Light and Distribution of the Fixed Stars. Mr. J. I. Plummer.

On two "Flats" on the Moon's Limb, observed March 23, 1877. Dr. R. Copeland.

Corrections to Struve's Double Stars contained in *Dun Echt Observatory* Publications, Vol. I. Lord Lindsay.

Nov. 9. Observations of *α Centauri*. Mr. R. L. J. Ellery.

Supplementary Note on the Theory of the Horizontal Photoheliograph. Prof. W. Harkness.

Observations on Lunar Eclipse, August 23, 1877. Rev. S. J. Johnson.

Mémoire sur la période commune à la fréquence des Taches Solaires et à la variation de la declinaison magnetique. Dr. R. Wolf.

Measures of *α Centauri*. Mr. H. C. Russell.

Note upon the proper Motion of *Groombridge* 3511. Prof. T. H. Safford.

Remarks to accompany Drawing of *Mars*. Mr. G. D. Hirst.

Total Lunar Eclipse, August 23, 1877. Mr. H. Pratt.

Note on the Eclipse of the Moon, August 23, 1877. Mr. J. E. H. Peyton.

Phenomena of *Jupiter's* Satellites 1875–77. Rev. S. J. Perry.

Lunar Eclipse, August 23, 1877. Rev. S. J. Perry.

Eclipses of *Jupiter's* Satellites in 1875, observed at Willet's Point, U.S.A. Mr. W. H Bixby.

On the Synodisms of *Saturn's* Satellites. Mr. S. M. Drach.

On three small Inequalities in the mean motion of the *Earth*, and an Inequality in the mean motion of *Mars*. Mr. E. Neison.

Note on the Second Catalogue of Micrometrical Measures of Double Stars made at the Temple Observatory, Rugby. Mr. J. M. Wilson and Mr. G. M. Seabroke.

On the relative Brightness of the details of *Jupiter's* Disk and Satellites. Prof. C. V. Zenger.

On the proper Motions of *Groombridge* 2275 and 2276 (Σ 1972). Mr. E. Dunkin.

Note on the supposed Physical Association of *Groombridge* 3477 and 3511. Mr. E. Dunkin.

A working List of Binary and other interesting Double Stars. Mr. E. Crossley and Mr. J. Gledhill.

On a New Term of Long Period in the mean Motion of the Moon. Mr. E. Neison.

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On an Arrangement of the Spectroscope for the observation of Objects of considerable Angular Diameter. Lord Lindsay.

Observations of the Lunar Eclipse, August 23, 1877, made at Morges, Switzerland. Mons. V. Fasel.

Expedition to Ascension. Preliminary Report. Mr. D. Gill.

Expedition to Ascension. General Report and duplicate copies of Observations, etc. Mr. D. Gill.

On the Physical Condition of *Mars*. Mr. J. Brett.

Note on the Spectrum of the Eclipsed Moon. Captain W. Noble.

Observations of the Satellite of *Neptune*. Mr. J. Ward.

On the Atmospheric Lines between D_1 and D_2 in the Spectrum. Mr. H. C. Russell.

Physical Observations of *Mars* made at the Royal Observatory Greenwich. The Astronomer-Royal.

On the Spectrum of a Solar Spot observed at the Royal Observatory, Greenwich. The Astronomer-Royal.

Elements and Ephemeris of Coggia's Comet. Prof. C. Pritchard.

On the Law of Force tending to any point whatever in the plane of motion in order that the Orbit may always be a Conic. Mr. J. W. L. Glaisher.

On the Motion of the Moon's Node in the case when the Orbits of the Sun and Moon are supposed to have no Eccentricities, and their Mutual Inclination is supposed to be indefinitely small. Prof. J. C. Adams.

Observations of *Mars* at Madeira, August and September 1877. Mr. N. E. Green.

A new Astronomical Clock. Mr. C. V. Boys.

On the inferences for the value of mean Solar Parallax and other elements deducible from the Telescopic Observations of the Transit of *Venus* 1874, December 8, which were made in the British Expedition for the observation of that Transit. The Astronomer-Royal.

Dec. 14. Ninth Catalogue of new Double Stars discovered with the 6-inch Refractor. Mr. S. W. Burnham.

Note on a special Case of "the most probable Result" of a number of Observations. Mr. J. M. Wilson.

On *Jupiter's* Third Satellite. Mr. J. Tebbutt.

Notes on *Mars*, 1877. Mr. H. Pratt.

Expedition to Ascension. Report, November 11, 1877. Mr. D. Gill.

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Extract of a letter to Mr. Lynn relating to a remarkable Solar Spot. Rev. F. Howlett.

Note upon the Determination of the Longitude of Kurrachee and Madras. Major.-Gen. T. Addison.

1878. Opposition of *Mars*, 1877. Mr. Maxwell Hall.

Jan. 11. On a cause for the appearance of Bright Lines in the Spectra of irresolvable Star Clusters. Mr. E. J. Stone.

Total Lunar Eclipse, August 23, 1877, observed at Cadiz. Señor A. T. Arcimis.

Suspected repetition or second outbursts from Radiant points, and on the long duration of Meteor showers. Mr. W. F. Denning, and remarks by Capt. Tapman.

On the probable errors of Transits of the first and second limbs of the Sun, observed by the Chronographic Method. Mr. A. W. Downing.

Note on the Transit of the Shadow of *Titan*. Lord Lindsay.

Sur les mouvements réels des Étoiles dans l'Espace. Señor V. Ventosa.

Ephemeris for determining the positions of the Satellites of *Uranus*. Mr. A. Marth.

On the correction of the Elements of *Ceres*. Mr. W. Godward.

Note on the Satellites of *Mars* and *Saturn*. Mr. A. A. Common.

The Sun-spot Period. Mr. B. G. Jenkins.

On a secular term in the mean Motion of the Moon. Mr. E. Neison.

On a small term of long period in the mean Motion of the Moon. Mr. E. Neison.

On a Cyclonic arrangement of the Solar Granules. Mr. W. Huggins.

On a combined Position and Setting Circle, rendering the Declination Circle unnecessary in large Newtonian Equatoreals. Mr. Wentworth Erck.

Expedition to Ascension. Report, December 3, 1877. Mr. D. Gill.

The center of Gravity of the apparent disk of a planet. Prof. A. Hall.

Note on Specular reflection from *Venus*. Mr. W. H. M. Christie.

On the evidence of periodic variability of the Nebula H. II. 278. Prof. A. Winnecke.

On the variable star U *Geminorum*. Mr. G. Knott.

On the distribution of Fixed Stars in Space. Mr. S. Waters.

Note on a Spectroscope made by Mr. Grubb for Prof. Young. Mr. Wentworth Erck.

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List of Public Institutions and of Persons who have contributed to the Society's Library &c. since the last Anniversary.

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The Board of Trade.
The Committee of Council on Education.
The Government of the United States of America.
The Meteorological Office.
Royal Society of London.
Royal Society of Edinburgh.
Royal Geographical Society.
Royal United Service Institution.
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Royal Society of New South Wales.
Royal Society of Tasmania.
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ADDRESS.

Delivered by the President, Mr. Huggins, on Presenting the Gold Medal of the Society to Baron Dembowski.

Before giving effect to the award of your Council by the formal presentation of the Medal to Baron Dembowski through the hands of our Foreign Secretary, you will permit me to place before you, in a few words, some of the considerations which induced your Council to bestow upon Baron Dembowski this high mark of their consideration.

It may be, perhaps, the more necessary on the present occasion for your President to make a statement of the grounds upon which the selection of your Council was based, because the work of your Medallist has not been of that original and remarkable character which carries within itself obvious justification for the highest honour which can be conferred upon it. When I say this, do not suppose me to admit that such work as that with which Baron Dembowski has enriched Astronomy is less deserving of honour and reward than are those direct and definite additions to our knowledge which we call discoveries. As in the case of a perfect building no wise man would attempt to apportion the relative values of the different parts, so in Science the deep foundations of accumulated measures and data are not less necessary, to say the least, than are the arches and pillars and towers of brilliant discoveries, upon which, doubtless, the glances of the mere passers by chiefly fall. It is as the diligent and laborious hewer and layer of the foundation stones of accurate measures and data that I desire to present the claims of your Medallist of to-day.

With untiring energy and a zeal which has never failed, he has pursued for twenty-five years that part of Astronomy which he at first selected as most completely within his power of accomplishing, the micrometrical remeasurement of the stars of the *Dorpat Catalogue*. Such steady work so faithfully persevered in never fails of success nor of reward.

The goddess of truth is always to be won by the worshipper who brings the three gifts, accuracy, energy, and perseverance. These are the gifts which your Medallist pre-eminently has placed in the service of our science and which have won for him most rightly the award of our Medal.

You will not think that I speak too strongly when I remind

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you, that notwithstanding that his instrument, of the very moderate aperture of five inches, was unprovided with clock motion, and had not even a position circle attached to it, he boldly at once undertook a revision of the brighter stars of the *Dorpat Catalogue*, and made up for the deficiencies of his instrument by his own skill so successfully that his measures, taken under these unfavourable circumstances, have come to be acknowledged by all workers in the same field as worthy of the greatest confidence. His energy and perseverance in the diligent pursuing of this one thing has enriched science with a long connected series of measures which are strictly comparable among themselves and extend without a break over a period of twenty-five years. May I venture to remark that it is, in my opinion, such quiet unobtrusive work which it is pre-eminently the duty of the Council, from time to time, to seek out and to honour. Great discoveries bring their own reward in the general attention and recognition which they at once claim and obtain for those who make them. But those who labour among the foundations in the accumulation of measures for other men to build upon are often but insufficiently remembered and honoured. In conferring upon Baron Dembowski the honour of which he has made himself so abundantly worthy, your Council wished at the same time to bestow a mark of encouragement upon the class of workers represented by your Medallist, who are content to pass their lives in quiet, accurate work, seeking no applause from the outside world of men, and eager only to lay their foundation stones truly plumbed and squared.

I ought not to omit to mention that the mode of publication of his observations adopted by Baron Dembowski, though it rendered his measures at once available to other workers, is unfavourable to a due appreciation of the magnitude of his work. If all his observations, which are now scattered through some seventy numbers of the *Astronomische Nachrichten*, were to be collected in one volume, the catalogue would not be unworthy to stand beside the most valued and extensive catalogues of double stars which we possess.

The first published work of Baron Dembowski appeared in the *Memorie della Reale Accadémia delle Scienze*, Naples, 1857. The same communication in French is to be found in the *Astronomische Nachrichten*, Nos. 999–1125.

This work consists of micrometrical measures of 127 double and triple stars selected from Struve's *Dorpat Catalogue*. The measures are preceded by an introduction, which contains a description of the instruments and methods of observation. The telescope was a dialyte of Plössl of 5 inches aperture and 5 feet focal length, mounted equatoreally, but unprovided with clock motion, and the position circle, which forms usually an essential part of the measuring apparatus. The reader is not surprised to meet with the remark that the moving of the hour circle by hand while the measures were made was often very

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difficult and sometimes painfully irksome. Should I incur much risk of being mistaken if I were to hazard the suggestion that these very difficulties, by the skill and perseverance which they developed to overcome them, contributed in some degree to the high quality of Baron Dembowski's work? Such severe training in the surmounting of serious instrumental difficulties is favourable to the formation of a masterly power and accuracy which is not always to be found among observers who possess from the first, ready to hand, every luxury of instrumental aid.

In the absence of a position circle Baron Dembowski made use of a simple practical method to find the angles of position from the points of intersection of the stars by their diurnal motion with the wires of the micrometer fixed at right angles to the moving wire. In speaking of his measures of distance, Baron Dembowski says: "Of 82 stars in which Struve did not recognise any sensible change of distance, my measures are in the mean about 0″·08 larger than those of the *Dorpat Catalogue*." Each measure in distance in the catalogue is the mean of 10 measures taken on the same day, and each measure of position is the mean of 5 independent measures on the same night.

The great care bestowed by Baron Dembowski to obtain the utmost accuracy possible under the circumstances in his measures may be illustrated by the following remarks in reference to angles of position: "In finding parallelism I invariably followed the direction of Struve by observing with the head in a vertical position. In some cases, when the angle between the imaginary line joining the components, and the vertical is more than 30°, I have verified the measures in three positions of the head, vertical, parallel to the line joining the components and perpendicular to the same." Baron Dembowski has added in the Catalogue a column containing the angle formed by the line joining the stars with the vertical.

This paper was followed by a communication to the *Astronomische Nachrichten* in 1859 (Nos. 1185–1209), which contains the completion of his re-examination of the *Lucidæ* of the *Dorpat Catalogue*, with the exception of those which were not within the reach of the instrumental means he then possessed.

In 1860 he contributed to the *Astronomische Nachrichten* the mean positions of 52 double stars taken at Naples between December 1855 and May 1858. These positions were obtained with a meridian circle of Starke, of 14 inches diameter, read with four microscopes, and provided with a telescope of 50 inches focal distance and 42 lines aperture.

Argelander made use of these observations in his "Untersuchungen uber die Eigenbewegungen von 250 Sternen," in volume 7 of the *Bonner Beobachtungen*.

In 1864 a new series of measures of 180 of the principal double stars of the *Dorpat Catalgoue*, including all those in which Dembowski suspected change, appeared in the *Astronomische Nachrichten* (No. 1473). He was now in the possession of a much

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more powerful instrument, and one in every respect adapted to his line of work, a refractor by Mertz of great optical excellence, 7 inches in aperture, and provided with clockwork and a suitable micrometer and position circle.

The earnestness with which Baron Dembowski sought to attain the greatest precision possible to him in his measures comes forcibly into view in the following sentence from his introduction to this new series of measures:—

"Among the numerous causes of inaccuracy in measures of position, I ought to mention one which exists in my own case, and which, so far as I know, is new. This source of error exists in the differences which I have often found in the values of the same angle according to the direction in which the position circle is rotated to bring the wires to parallelism with the line joining the two stars. I have often found that a position of the wires which was at the time considered satisfactorily parallel was in reality towards the side from which the movement of rotation was given."

On this account, Baron Dembowski made it a rule to take an even number of measures with the circle alternately moving in opposite directions. He makes the suggestion that this peculiarity of judgment, which may be stronger in some observers than in others, may be one of the causes which contribute to the want of agreement in the angles of position of the same objects at the same epoch among the measures of different observers. Mr. Dawes states, in the introduction to his Catalogue of Double Stars, that he had detected in himself the same tendency of his eye to accommodate its judgment to the position of the wires before they had been quite brought up to the right point. He then says: "I was much interested in some remarks on this subject by that excellent and indefatigable double-star observer, Baron Dembowski.

In the *Astronomische Nachrichten*, 1866 (No. 1572), Dembowski gives a further series of measures of about 150 stars.

Again, three years later, in a communication to the *Astronomische Nachrichten* (Nos. 1735, 1736), he brings to completion his revision of the *Dorpat Catalogue*, leaving not more than 64 stars out of the 2,641 stars of the Catalogue, which for various reasons he had not been able satisfactorily to measure. Baron Dembowski adds: "This result is the highest praise that can be given to the excellence of Mertz's object-glass." May we not even more justly say that it is at the least an evidence as remarkable of the skill and zeal of the observer?

Our Medallist did not, on the completion of the task he had set himself, rest from his labours; for, in the following year, we have another series of measures of some of the principal double stars, and also a first series of measures of stars from the *Pulkowa Catalogue* (*Astronomische Nachrichten*, 1798–1832).

This was followed by other measures which appeared in the *Astronomische Nachrichten* (1886–1888).

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In 1873 Baron Dembowski communicated to the *Astronomische Nachrichten* (1936) an elaborate determination of the value in arc of the revolutions of his micrometer.

A further series of measures of the principal double stars, and of 8 new double stars, made during the years 1872 and 1873, are contained in No. 1979 of the *Astronomische Nachrichten* (March 1874).

In July of the following year (*Astronomische Nachrichten*, No. 2045) Baron Dembowski calls attention to the remarkable changes which have occurred in the triple star 503 of Sir James South's list. He thinks there can be no doubt of the identity of this object with his double star Pulkowa 124. In this case the movement of—

AB in 49·5 years will be $-32''{\cdot}4$ and $-14°{\cdot}7$,

AC in 50·1 years will be $+28''{\cdot}3$ and $-1°{\cdot}6$.

The measures taken of the principal double stars during the years 1874 and 1875 were communicated to the *Astronomische Nachrichten* in 1876 (Nos. 2076–2086). This series includes also measures of 33 stars of Mr. Burnham's Catalogue which do not exceed 20° of south declination.

Lord Lindsay—May I ask you to convey this Medal to Baron Dembowski, with the assurance of our high appreciation of his accurate and long-continued work, and of our earnest wish that he may have health still to continue his labours for many years to come.

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The Meeting then proceeded to the election of the Officers and Council for the ensuing year, when the following Fellows were elected:—

President.

LORD LINDSAY, M.P.

Vice-Presidents.

ARTHUR CAYLEY, Esq., M.A., LL.D., F.R.S., Sadlerian Professor of Pure Mathematics, Cambridge.
EDWIN DUNKIN, Esq., F.R.S.
WILLIAM HUGGINS, Esq., D.C.L., LL.D., F.R.S.
WILLIAM LASSELL, Esq., LL.D., F.R.S.

Treasurer.

FRANCIS BARROW, Esq., M.A.

Secretaries.

J. W. L. GLAISHER, Esq., M.A., F.R.S.
A. COWPER RANYARD, Esq., M.A.

Foreign Secretary.

J. R. HIND, Esq., F.R.S., Superintendent of the *Nautica Almanac.*

Council.

J. C. ADAMS, Esq., M.A., LL.D., F.R.S., Lowndean Professor of Astronomy, Cambridge.
Sir G. B. AIRY, K.C.B., M.A., LL.D., D.C.L., F.R.S., Astronomer Royal.
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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. MARCH 8, 1878. No. 5.

LORD LINDSAY, M.P., President, in the Chair.

John E. Gore, Esq., C.E., M.R.I.A., Asst. Engineer, D.P.W., India, Dromard Rectory, Co. Sligo;

The Rev. Frederic John Hall, Haileybury College, Hertford;

William Hobson, Esq., Park View, Whalley Range, Manchester;

Lieut. Robert Hoggan, R.N., H.M. Dockyard, Sheerness;

William C. Otter, Esq., 60 Grosvenor Park, Camberwell; and

F. H. Stevens, Esq., B.A., Clifton College, Clifton;

were balloted for and duly elected Fellows of the Society.

Letter received by Lord Lindsay from Dr. W. Foerster.

MY LORD,—As the time is approaching when the Annual Report of the Council of the Royal Astronomical Society is published, I beg to call your attention to a passage in the last Report (February 1877, p. 188), where the planetary work of the *Berliner Astron. Jahrbuch* is mentioned in a way which does not do full justice to it.

It is said in this passage that space has only been found in the *Berliner Astron. Jahrbuch* for 50 out of 125 planets which will be in opposition between January 1 and December 31, 1877, "and it is regretted that so few of the older and larger planets are included in the selection."

From these expressions it will be inferred that the *Berliner Jahrbuch* has been liable to certain external and, as it seems by the words "space has not been found," to economical restrictions which have not permitted the superintendents to do the necessary planetary work required by science.

But in every volume of the *Jahrbuch* since the year 1874, and finally in *Circular* No. 44, we have endeavoured to

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show by general deductions, accompanied with more and more detailed practical propositions, which have nowhere been seriously refuted and which have been approved by several meetings of astronomers, that the most perfect and exhausting care of the small planets is of prominent scientific interest, and that in the actual state of things this task can only be reasonably fulfilled by the introduction of some restrictions of a scientific nature, but in no way of economical or any other external character.

The intensity of planetary work at Berlin and in the whole astronomical world has not weakened, but has sensibly increased, since these restrictions have been carried into practice. Most astronomers concerned in that department of science have now gained the hope that, by reasonable organisation, and discernment of the essential from the secondary or imaginary wants of science, the whole planetary work in future may be fully mastered, and that thus, by an exhaustive treatment according to our propositions, more and more important results will be furnished for the small planets. It will be seen by the Royal Astronomical Society—especially from the periodic *Circulars* of the *Berliner Jahrbuch*, which now form an essential completion of the annual volume of this publication—that the Superintendency of the *Jahrbuch*, mainly conducted by my friend, Dr. Tietjen, endeavours to do the best possible to aid observers and computers of small planets.

I hope that you will be able to procure for this communication a place in the next publication of the Royal Astronomical Society.

I am, my Lord, with the greatest respect, your obedient servant,

W. Foerster,
Director of the Berlin Observatory.

Berlin,
1878, *February* 10.

Galileo's Trial before the Inquisition in the light of recent Researches.

By Sedley Taylor, M.A., late Fellow of Trinity College, Cambridge.

(*Communicated by J. W. L. Glaisher, Esq.*)

Within the last ten years a large number of previously inaccessible official documents relating to the trial of Galileo before the Inquisition have been made public. The fresh evidence thus far produced has given rise to an entirely new view as to the nature of that proceeding, a short account of which will be found in the following Paper:—

Up to the year 1867, the best informed writers held, almost.

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to a man, that the Inquisition, whatever might be thought of the wisdom of its proceedings, had a clear legal right to punish the great astronomer for the course which he pursued. The text of the sentence against him stated that he had committed a palpable contempt of court by writing in favour of the Copernican doctrine in 1632 after having in 1616 been formally inhibited by the Inquisition from doing so. As long as this assertion was accepted it was obviously impossible to clear Galileo of the charge of contumacy, by which his condemnation was in the last resort determined. It is a singular circumstance that historians, many of whom were by no means antecedently inclined to repose a childlike faith in the veracity of the Inquisition, did, one and all, accept this its unsupported assertion, and with equal unanimity overlooked a glaring contradiction on the surface of the sentence, which it was reserved for a German writer to point out in 1870.

The view that Galileo was legitimately punished for contempt of court received much support in 1850 from a work published by Marino Marini, Prefect of the Secret Archives of the Holy See, and entitled *Galileo e l' Inquisizione*. It was previously known that a contemporaneous record of the trial, drawn up from day to day by the Notary of the Inquisition, was preserved among its archives: Marini, the official guardian of this record, now for the first time printed a considerable number of extracts from its contents. The object of this writer manifestly was to represent the conduct of the court as conspicuously mild and benignant; that of the accused as intolerably troublesome and rebellious. By a system of really masterly suppression and garbling of the documents to which he alone had access, Marini succeeded in completely misleading historians of the trial for more than a quarter of a century, until, in fact, the publication *in extenso* of the documents manipulated by him exposed his manipulations to the light of day.

The first tolerably complete edition of the trial-record was published in 1867 by *M. de l'Epinois*, to whose latest exertions a few months ago we owe a verbatim issue of the whole. Professor Berti also contributed an edition in 1876, and Herr v. Gebler closes the series with one still more recent, and aiming at even more minute reproduction of the original than its predecessors. The evidence supplied by the official minutes of the trial is therefore henceforth universally accessible except in so far as the genuineness of parts of the manuscript itself may be called in question.

The writer who has made the most important use of these new materials is Dr. Emil Wohlwill, of Hamburg, who published, in 1870, a short work on the trial, and followed it, late in 1877, by a detailed examination of the question whether the prisoner was submitted to the torture during the course of the proceedings. The earlier of these publications investigates an issue of much greater, indeed of cardinal, importance, viz.

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whether the Inquisition told the truth when it asserted that Galileo had, in 1616, been formally served with an order of court inhibiting him from defending the Copernican doctrine. Wohlwill maintains that this asserted inhibition had no actual existence, and that the whole incident was a fraudulent fabrication perpetrated in order to secure a condemnation which could not be compassed by legitimate means. That a forgery was committed in one of the documents of the trial-record for the purpose of carrying out this nefarious scheme, and that highly placed officials of the Inquisition were accomplices in the conspiracy, are corollaries which follow immediately from the above position.

Before stating the chief arguments on which the German writer relies, it will be well to premise a concise *résumé* of the essential facts in Galileo's case.

It is probable that about the year 1592 he became a convert to the heliocentric system, which had been propounded half a century before by Copernicus in the *De Revolutionibus Orbium Cœlestium*. He seems, however, for a considerable time after this change of view, to have continued publicly to teach the Ptolemaic system, and this, as he himself explained in a letter to Kepler, "on account of the huge number of fools" and the derision and molestation which adherents of the new doctrine would be sure to experience at their hands. But by slow degrees he must have allowed his genuine convictions to appear, for in 1615 we find him furiously attacked from the pulpit for his adherence to the new astronomy. The preacher, a monk at Florence, denounced mathematics as a diabolical art, and sought to enlist the force of ridicule on his side of the question by quoting the verse at the opening of the Acts of the Apostles—

> Ye men of Galilee, why stand ye gazing up into heaven?

as a punning hit against the star-gazing disciples of Galileo. It ought, perhaps, in justice to this monastic punster, to be remarked that his joke sounds decidedly better in the Latin of the Vulgate than it does in the English of the Authorised Version. "Galilei," the Latin equivalent for "Galileans," is identical with the proper surname of the philosopher, whose complete designation was Galileo dei Galilei. By some odd freak of custom we in England invariably call him by his Christian name only.

The enemies of our astronomer unfortunately did not confine their mode of warfare to sermons and Biblical puns—an artillery whose fire he no doubt was prepared to sustain with considerable equanimity. The monks of the monastery of St. Mark concocted a letter to the Inquisition at Rome, in which they denounced to that formidable tribunal some opinions which Galileo really did hold, and a great many more which he did not. The Holy Office, once put on the scent of heresy, set to work with its usual keenness, and before long Galileo received a private hint from friends in Rome that some underground machination was going on

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against the new astronomy. On this intimation he went up to the capital, in the hope that, by pointing out to the authorities the futility of attempting to arrest the progress of the new views by ecclesiastical censures, he might save the rulers of the Church from the humiliation of publishing decrees against the laws of nature. Galileo overestimated his powers of persuasion. The absurdity which he strove to avert was resolved upon, and the formal decree, declaring the Copernican doctrine to be false and contrary to Scripture, was issued by the Congregation of the Index on the 5th of March 1616.

It is very generally supposed that by this decree the work of Copernicus was prohibited, but this is a misconception. The *De Revolutionibus* was only ordered to be "suspended until corrected," i.e. withdrawn from circulation for the purpose of having certain alterations made in its text under the direction of the Congregation of the Index. The list of corrections subject to which the work was permitted to reappear was published in 1620; it is very short, covering, perhaps, two pages out of two hundred. The object aimed at by the correctors was manifestly the introduction of *hypothetical* language where the author had spoken *categorically*. Much skill is shown in giving this turn to a number of passages by the slightest verbal alterations, and it is only a very few paragraphs which, as not susceptible of such treatment, were ordered to be expunged. The fact that the treatise of Copernicus was allowed to reappear with only these superficial readjustments, which left its long chain of reasoning untouched in a single essential link, shows conclusively that the Congregation, though determined to get rid at a single stroke of the vexatious discussions likely to arise about the relations between Scripture and the new astronomy, did not demand more than a certain decorum of overt expression from writers who discussed the question from a purely scientific point of view.

In 1632 Galileo published his Dialogues on the Ptolemaic and Copernican systems, which led directly to all the misfortunes and sufferings of his remaining years. The work was suspended about seven months after its appearance by a peremptory order from Rome, and two months later its author received a citation to appear before the Supreme Tribunal of the Inquisition. The result of the subsequent trial is well known. The accused was declared to have rendered himself vehemently suspected of heresy, compelled to abjure, and condemned to incarceration during the pleasure of the Court: a punishment which was, however, promptly commuted to practical imprisonment in his own remotely situated villa at Arcetri, where he dragged out his remaining years under the humiliating *surveillance* and repeated menaces of the Holy Office. The main charges on which the prisoner was convicted were the following:—

(1) That the Dialogues transgressed the limits laid down by the decree of the Index.

(2) That they were in violation of a private personal in-

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junction, containing restrictions far more stringent than those of the public decree, which the tribunal asserted to have been formally delivered to Galileo on February 26, 1616, by the then acting Commissary-General of the Holy Office.

To the first allegation the prisoner had a complete answer. He had submitted his work in manuscript to the Papal licensing authority, and obtained the regular *Imprimatur* before proceeding to publication, and therefore could not be made criminally responsible for its contents, inasmuch as it was manifestly not his business, but that of the licenser, to know what was and what was not compatible with the decree of the Index.

The second article of charge depends, of course, on the exact terms of the notice given to Galileo on February 26, 1616. The nature of the whole asserted proceeding appears from the following entries, translated from the Latin of the trial-record :—*

"Thursday, Feb. 25, 1616.—The Most Illustrious Lord Cardinal Millinus gave notice to the Rev. fathers, the Lords Assessor, and Commissary of the Holy Office that, on the censure of the Fathers Theologians passed upon the propositions of Galileo—that the Sun is the centre of the universe and locally motionless, and that the Earth moves even with a daily motion—being reported, the Most Holy ordered that the Most Illustrious Lord Cardinal Bellarmine should summon to his presence the said Galileo, and admonish him to abandon the said opinion, and that, if he should refuse to obey, the Father Commissary, in the presence of a Notary and Witnesses, should deliver to him a precept entirely to abstain from teaching or defending such doctrine or opinion, or from treating thereof; but that if he did not acquiesce he was to be imprisoned.

"Friday, Feb. 26 of same year.—In the Palace habitually occupied by the said Most Illustrious Lord Cardinal Bellarmine, and in the apartments of his Most Illustrious Lordship the above-named Galileo having been summoned, and himself being in the presence of his Most Illustrious Lordship, and in the presence of the very Rev. father Brother Michael Angelo Seghitius of Lodi, of the Dominican order, Commissary-general of the holy office, the Same Most Illustrious Lord Cardinal admonished the above-named Galileo concerning the error of the above-named opinion, and to abandon it; and thereupon and forthwith, in the presence of myself and Witnesses, and the same Most Illustrious Lord Cardinal being likewise still present, the *Father Commissary enjoined upon and ordered the aforesaid Galileo, he being still in the same place and under formal Presentment, in the name of Our Most Holy Lord Pope and of the whole Congregation of the holy office, entirely to abandon the above-named opinion that the Sun is the centre of the universe and motion-*

* The paragraphs thrown into italics are those which Wohlwill considers to be a subsequent and fraudulent addition.

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less and that the Earth moves, and in Future not to hold, teach, or defend it in any manner whatever, by word of mouth or in writing, failing which proceedings would be taken against him in the Holy office; to which precept the same Galileo submitted and promised obedience.—Done at Rome as above, there being present at the same place R. D. Badinus Nores of Nicosia in the Kingdom of Cyprus, and Augustinus Mongardus, of a place in the Abbacy of Rose, in the diocese of Montepulciano, servants of the said Most Illustrious Lord Cardinal, as Witnesses."*

The above minutes are quoted verbatim in the sentence of 1633, as first-hand evidence of what occurred in 1616, and the whole incident of the Commissary's inhibition rests on no other authority. Everything therefore turns on the question whether this incident was a fact or a fiction. Wohlwill's reasons for adopting the latter of these alternatives shall next be summarily stated.

An unsuspecting reader might easily take the minute of February 26 as describing a straightforward carrying out of the order laid down in that of February 25. It can, however, be so regarded only by making an arbitrary assumption which will presently be shown to be contradicted by positive evidence. The Papal order contained in the first minute provides that the Commissary was only to intervene officially on the occurrence of a particular contingency, viz. on Galileo's refusing to obey Bellarmine's admonition. The second minute represents the Commissary as delivering his "precept" immediately upon the conclusion of the Cardinal's address, and, as the language used seems to imply, without even waiting to ascertain whether Galileo refused to obey or not. It is impossible to suppose that the Commissary would have dared, in the presence of the leading Cardinal of the Holy Office, thus to set at nought the direct instructions of the Pope himself. If therefore the two minutes are to be reconciled at all, this can only be done by assuming, without a tittle of evidence, that Galileo, when admonished by the Cardinal, refused to submit, and so brought down upon himself the Commissary's precept.

But this assumption is put out of court by a piece of direct evidence which came to light under singularly interesting circumstances. In 1848, when the suspension of the temporal power at Rome placed the secret archives of the Vatican for a brief interval within the grasp of the revolutionary government, Professor Silvio Gherardi, one of the Secretaries of State, seized the opportunity to institute a search among the records of the Inquisition with a special eye to documents bearing on the case of Galileo. After an extremely laborious exploration he succeeded in unearthing a considerable number, which, as subsequent comparison has shown, were not included in the collection since published, and probably formed part of an entirely distinct series.

* The text of *v. Gebler's* edition has been followed.

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One of these documents relates how, on March 3, 1616, Cardinal Bellarmine reported as follows to the Pope:—"Galileo Galilei, mathematician, admonished by order of the Sacred Congregation to abandon the opinion which he has hitherto held, that the Sun is the centre of the spheres and immovable, but the Earth movable, acquiesced."

No words are needed to enforce the decisive bearing of this pregnant sentence. It is evident that Bellarmine is here reporting his execution of the order contained in the minute of February 25, and its successful result. Galileo had acquiesced under his admonition, and the contingency which was to entail the Commissary's intervention had not arisen. The attempt to reconcile the conflicting minutes by assuming that Galileo refused to submit must therefore be abandoned. It will be noticed how Bellarmine's report corroborates the minute of February 25 and the earlier part of that of February 26, while it invalidates the latter's disputed close.

Singularly enough, the great Cardinal has left behind him a further piece of testimony equally destructive of the *rôle* attributed to the Commissary. Having been applied to by Galileo, a few months after the interview, for a memorandum stating exactly what had occurred, Bellarmine forwarded an autograph certificate that only the declaration published by the Index Congregation (i.e. their decree of March 5 on the Copernican theory) had been announced to Galileo. It is manifest that this statement entirely excludes the delivery to him of a further and infinitely more stringent prohibition, such as the Commissary's precept. The Court in their sentence quoted Bellarmine's certificate as genuine and authentic, and by so doing implicitly invalidated the asserted action of the Commissary, on which, nevertheless, they persisted in condemning the prisoner. It is this flagrant contradiction between two pieces of evidence, each insisted on by the Inquisition as trustworthy, to which reference has already been made as having been for the first time pointed out by Wohlwill.

The evidence given by Galileo during his examinations by the officials of the Holy Office, and the statements of his written defence handed in to the tribunal, minutely confirm the position that no such prohibitory order as the precept of 1616 had ever reached him. In the interrogatory of April 12, 1633, the efforts of the examining official were specially directed towards eliciting an admission of the delivery on February 26, 1616, of the Commissary's precept. The examiner manifestly held in his hand the minute the genuineness of which is now called in question, and sought, by reading aloud portions of its text, to recall the incident it described to the memory of the accused. His attempts were fruitless. Galileo, though plainly anxious to show the utmost docility to the wishes of the Court, and willing to accept the technical term "precept" ("*precetto*"), by which the examiner designated the communication made to him, asserted that he had no recollection of the precept in question having

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been delivered to him by any other person than Cardinal Bellarmine.

In his defence, presented to the Court on May 10, Galileo remarks that the authentic attestation of what took place on February 26, 1616, which he possessed in Bellarmine's certificate, did not contain a trace of an indication that any injunction had been laid upon him save that general pronouncement which concerned everyone alike—viz., of course, the decree of the Index.

These statements appear to be supported by the strongest internal probabilities. We may at once set aside the notion that Galileo may have *forgotten* the Commissary and his message. His detailed statements on the multifarious subjects as to which he was examined show that he possessed a memory of unusual tenacity. When we think what in Italy, in the seventeenth century, it was in the power of the Inquisition to inflict on those who incurred its displeasure, we cannot doubt that, had Galileo been actually brought face to face with a leading official of the dreaded tribunal, and received from him the order of the Court, ending with the warning "Obey, or a suit will commence against thee in the Holy Office," the scene must have been branded into his memory in letters never to be effaced while life and memory endured.

It may indeed be urged, and has been urged, that Galileo, though conscious that he had received the Commissary's precept, chose deliberately to deny it. Now as, unfortunately, it cannot be denied that, at a later point of his examination, Galileo was betrayed into asserting on oath what was unquestionably false, there is nothing antecedently improbable in the suggestion that he may be doing the same thing here. Nevertheless there are, I think, indications which point decisively to an opposite conclusion.

We may assume that a sensible man, to say nothing of one of eminent tact and acuteness, will not, however unscrupulous he may be, tell a deliberate lie by which little or nothing is to be gained, especially if by doing so he is likely to expose himself to serious and imminent peril. Now Galileo, before advancing the statements which we are discussing, was made aware by the official who examined him that the Court was in possession of a formally attested record of what took place on February 26, 1616. He must have felt, therefore, that it was useless to deny what thus admitted of direct legal proof—and not merely useless, but excessively dangerous; it being the practice of the Inquisition to resort to the application of the torture when there was reason to suspect that a witness was not telling the truth. For these reasons, if Galileo had been conscious of having really received the Commissary's precept, he would surely not, in the face of the official minute which he saw in his interrogator's hand, have ventured on the desperate course of denying all knowledge of it.

There is a further argument to which considerable weight ought

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to attach. A person who reads through the whole of Galileo's answers on April 12 will hardly fail to reach the conclusion that he did not really know what his examiner meant by the "precept," and fancied he alluded to a particular form of words employed by Cardinal Bellarmine, and that the question was whether or not certain crucial expressions, such as "to *teach*" and "in *any* manner," were or were not included. On the whole, the balance of probability inclines very decidedly in favour of the truthfulness of Galileo's asseverations. They contain, therefore, strong evidence for the non-delivery of the precept of 1616.

In addition to the direct evidence which has been already detailed, Wohlwill has collected a considerable body of collateral indications which tend to exclude the asserted action of the Commissary. Some of these shall next be briefly stated.

The fact that Galileo was a devout and warmly attached member of the Roman Church affords some antecedent presumption that he would not have refused to yield to an admonition from her greatest Cardinal, with whom too he stood on a footing of private friendship. But further, in his private letters he is found repeatedly congratulating himself on the docile attitude he had assumed towards the Church on the Copernican question. Thus, in a letter written on March 6, 1616, he says that his conduct throughout the whole transaction had been such that "no saint could have borne himself with greater reverence or greater zeal towards Holy Church." This is hardly the language of a man who but ten days before had met Bellarmine's admonition with a refusal to acquiesce.

The same asseverations reappear in a letter written some two years after his condemnation, when all hopes of softening the hearts of his persecutors by professions of zeal for the Church must, if he ever entertained them, have utterly vanished:—

"I have said," he writes, "that I do not hope for any alleviation, and this because I have committed no crime whatever: I might have hoped to obtain grace and pardon had I erred; for transgressions afford to princes the means of exercising mercy and indulgence; but in the case of a man unjustly condemned, the rigour of the sentence must be sustained in order to keep up an appearance of its having been legitimately arrived at. This afflicts me less than others may think, because two consolations constantly support me. One of these is that there is not to be found in all my writings the least shadow of lack of piety or of reverence for Holy Church. The other is my own conscience, fully known only to myself on earth and to God in heaven, who well knows that in the cause for which I suffer many might have acted and spoken with greater learning, but not one, even of the holy fathers, more piously nor with greater zeal towards Holy Church, nor, in short, with a more holy intention, than I."

From a letter of Galileo, dated March 12, 1616, we learn that on the previous day he had a most gracious audience of the

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Pope, who, in reply to some expressions of alarm let fall by Galileo at the prospect of being the object of malignant and implacable attacks, assured him that he was so well thought of by himself and by the whole Congregation that credit would not be lightly given to the persecutors.

If on February 26 Galileo had met the admonition of Bellarmine with a refusal to submit, and had thereupon been visited by the Commissary, "in the name of our lord Pope and of the whole Congregation of the Holy Office," with a peremptory and threatening inhibition, it is all but inconceivable that the Pope should, within a fortnight, have assured him how well he stood, "both with himself and the whole Congregation."

Galileo's subsequent conduct was moulded most carefully in conformity to the Index decree, but was quite inconsistent with the terms of the order put in the mouth of the Commissary. Thus, when in May 1618 he presented to the Archduke Leopold of Austria a copy of his treatise on the tides, he was careful to accompany it with a statement that, since it was becoming to obey and believe in the decisions of the authorities, he wished any argument of his which rested on a Copernican basis to be regarded as a piece of poetry or a dream. It is manifest that this, though a literal compliance with the demands enforced against Copernicus by the Index Congregation, was no compliance at all with a precept not to teach or defend heliocentricism "in *any manner*, in writing or by word of mouth."

In 1624 Galileo wrote an elaborate answer to a letter against the Copernican system which had been addressed to him in 1616 by a certain *Francesco Ingoli*, who had in the meantime become secretary to the Congregation of the Propaganda. Here again he explicitly disclaims any intention of maintaining and holding as true that proposition which had been declared inconsistent with doctrines more majestic and authoritative than those of the natural sciences, and says that his object in writing was to show that, when he was engaged with astronomers and philosophers on this question, he was neither so blind of intellect nor so feeble in argument as to have taken the Copernican side for lack of having perceived, or understood, the instances produced by Ingoli. He then administers a severe rebuke to the unlucky secretary, telling him that, for every day he had devoted to the subject in hand, Copernicus had devoted a year, and that he must not lightly imagine himself able to overthrow so great a man as Copernicus, especially with such weapons as those he employs against him, which are objections of the commonest and most trivial description. After this he sets to work deliberately to tear the poor man's arguments to pieces one by one, for some forty pages or more. A copy of this production was forwarded by Galileo to Ciampoli, secretary to Pope Urban VIII. (who had succeeded Paul V. in 1624). It is difficult to suppose that Galileo, with a threat of proceedings in the Holy Office hanging over his head, would have dared to send to the Pope's secretary

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such a barefaced violation of the precept delivered him by the Commissary.

Still more decisive is the history, too long to be gone through in detail, of the negotiations which preceded the publication of the great "Dialogues." Galileo unhesitatingly placed in the hands of the licensing authority at Rome a work, the very title of which claimed the right of discussing the Doctrine of Copernicus, subject only to the limitations enacted by the Index Congregation, to whose decree the preface contains a formal adhesion. The work, after being subjected to minute and reiterated inspection, received the final approval of the licensers, and appeared with their Imprimaturs conspicuously displayed on its first page.

The conduct of the licensers is perfectly intelligible on the assumption that the decree of the Index alone guided their scrutiny, but utterly unaccountable on the opposite supposition. The only possible object which the Inquisition could have had in laying Galileo under the special personal restrictions detailed in the Commissary's precept was to bind him over to absolute silence about the condemned doctrine. It seems highly improbable that, in a case of such importance, an order like that of the Commissary should never have been notified to the Pope's licensers of books, should have become a dead letter, and within sixteen years been so utterly forgotten as to have been only "discovered" in a search among the archives of the Inquisition, instituted after the appearance of Galileo's "Dialogues."

The general character of the collateral evidence will be clear from the above summary. There remains only one more point of great importance for consideration, viz. the indications of genuineness or spuriousness afforded by the state of the manuscript trial-record itself as far as regards the disputed document. In his new edition, M. de l'Epinois has given a photo-lithographic facsimile of the minutes of February 25 and 26, and thus enabled other persons besides the exceedingly select few who have as yet been permitted to inspect the originals to form some idea of the evidence presented by their present condition. In the opinion of Wohlwill the result of a careful examination strikingly confirms his view. The first part of the minute of February 26, down to the Latin word translated "whole" in the version given above (pp. 260, 261), occupies the lower half of a left-hand page (fol. 378) of the manuscript; the remainder of it the adjoining right-hand page (fol. 379). The last two lines of fol. 378, in which the intervention of the Commissary is narrated, present, when examined under a magnifying glass, certain very peculiar phenomena which occur in no other part of the facsimile. A number of dots, and what look like remnants of older writing, are seen peeping out between the letters, and parts of letters, of the present text, and there is a blurred appearance about certain of the words, and notably about the all-important word "Commissary," which looks very much as if it were the result of

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writing over an erasure. Wohlwill's theory, which he supports by an investigation too technical for reproduction here, is that the minute of February 26 originally terminated at the foot of fol. 378 with a statement that Galileo had acquiesced under Bellarmine's admonition. The last two lines which contained it were then, he thinks, operated on by partial erasure and re-writing, including the use where possible of some of the old words or letters, so as to bring them into their present form. The fraud was then easily completed by writing the rest of the spurious minute on the next page, which, like many others in the manuscript, may very probably have been originally left blank. In doing this the genuine handwriting was carefully imitated, but, as Wohlwill confidently maintains, with but incomplete success.

It is proper to remark here that two of the three editors of the manuscript, Berti and v. Gebler, assert the genuineness of the disputed minute, while the third, De l'Epinois, has as yet expressed no opinion on the point. This fact affords some presumption against the view of Wohlwill, which was arrived at without personal access to the impugned document. A thorough re-examination of its contents by skilled and trustworthy palæographers alone offers a prospect of settling the question.

It is obvious that this new view of Galileo's trial involves the gravest charge against the Inquisition, that, viz., of having manufactured a spurious basis for a criminal indictment, and consummated a cruel wrong on the ground of an iniquitous fraud. If this charge is once established, it will be no easy matter to exonerate even the Cardinals of the supreme Congregation itself from complicity in the crime.

Clearly, therefore, the authorities of the Vatican will do well, unless they desire judgment to go by default, to produce additional documentary evidence, and bring this plain issue, so deeply affecting the good faith of the Holy Office in by far the most famous case ever decided at its bar, to a definitive settlement. The object of the few remaining lines devoted to this subject here will be to show that important unpublished evidence in the case unquestionably once existed, and presumably still exists, among the archives of the Inquisition.

It has been made abundantly clear by the most recent researches that the published trial-record is far from complete, that some of its documents are only copies or abstracts, and that others show plain marks of a later origin than the rest of the collection. The sentence and form of abjuration are both missing, as are also the opinions given by the consulting jurists of the Inquisition, though it is known from the Court's own statement that these opinions were actually obtained. Further, there is no report of the sittings held on the case by the supreme Congregation. The short references to decisions there arrived at are mere unauthenticated summaries. The original formally attested records of all these must once have formed part of the Inquisitional archives, and ought to be forthcoming now.

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On Hansen's Terms of Long Period in the Lunar Theory.

By E. Neison, Esq.

Recent investigations have shown that the theory of the terms of long period in the motion of the Moon is at present in a most unsatisfactory condition, so that the entire subject urgently requires further and more extended investigation. When Hansen announced that he had discovered the existence of two terms of long period in the motion of the Moon, due to the perturbations of *Venus*, it was thought that the last difficulty in the lunar theory had been overcome. Hansen's tables were found to completely represent the motion of the Moon for the period 1750–1850, and this was regarded as the most satisfactory evidence of the correctness of the theory on which they were based.

Unfortunately, in a most important point Hansen's tables do not really rest on his theoretical investigations. Purely empirical values were assigned by Hansen to the coefficients of the two important terms of long period which he had discovered to be produced by the planet *Venus*. Referring to these terms, Hansen says: "La détermination exacte de ces deux inégalités par la théorie est la chose la plus difficile que l'on rencontre dans la théorie du mouvement de la lune. J'ai cherché deux fois à déterminer leurs valeurs, et par des méthodes différentes, mais j'ai obtenu des résultats essentiellement différents l'un de l'autre." (*Monthly Notices* vol. xv., Nov. 1854.) For this reason, therefore, Hansen thought it best to give such empirical values to these terms as was necessary to bring theory and observation into accord during the interval 1750–1850. In this respect, therefore, Hansen's tables are decidedly unsatisfactory, and the fatal consequences of this course have swiftly overtaken his tables. The values for the coefficients of these two terms of long period, which Hansen obtained from his theoretical investigations, appear to have differed considerably from the empirical values employed by him in his tables. Hansen has stated that he intended undertaking a third investigation for the purpose of satisfactorily determining the theoretical value of these two coefficients. The results of this new investigation have never been made known, and it is doubtful if it was ever completed.

In 1858 Delaunay was led to take up the examination of these two terms of long period in the motion of the Moon, which had been discovered by Hansen to be produced by the action of *Venus*, for he remarks the lunar theory could not be left in the unsatisfactory condition mentioned by Hansen. Delaunay arrived at most important results; for whilst he confirmed from theory the value which Hansen had empirically assigned to the coefficient of the term of long period with the argument $(g - 18g'' + 16g')$, using Hansen's notation (*Conn. des Temps*, 1862, App.), he found that the theoretical value of the coefficient of the other term, whose

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argument is $(8g''-13g')$, was so small that it might be regarded as insensible. (*Conn. des Temps*, 1863, App.)

This was a very serious matter; for if Delaunay's results were correct, they showed that there must be grave errors in the theoretical basis of Hansen's *Tables de la lune*. And if this were so, these tables could not continue for long to represent the place of the Moon. In 1870 Professor Newcomb pointed out that Hansen's tables failed apparently to represent the observations anterior to 1750, and he has lately confirmed this by a more extended investigation, in which he clearly shows that great discordances exist between the observed position of the Moon and the position assigned by Hansen's tables. What is of still more importance is the fact that of late the places of the Moon assigned by Hansen's tables have deviated very considerably from the observed place, and this deviation is rapidly increasing. It is evident, therefore, that although by giving empirical values to the coefficients of the two terms of long period which he had discovered to be produced by the perturbations of *Venus*, Hansen had been enabled to make his tables represent the observed place of the Moon during the interval 1750–1850; yet this had only been effected by means which had rendered his tables discordant prior and subsequent to this period.

It is certain, therefore, that there is some grave deficiency in Hansen's *Tables de la lune*. This fact strongly confirms the accuracy of Delaunay's conclusions with respect to the value of the terms of long period, but only to this extent, that Hansen's empirical values are wrong. For if Delaunay's values of these terms be adopted, and Hansen's tables be corrected, they still fail to represent the motion of the Moon, with this important difference, that they do not represent it for any period whatever. Either, therefore, both Hansen's and Delaunay's values are wrong, or there still remains to be discovered one or more important terms of long period in the theory of the Moon.

The determination of the value of the coefficients of the terms of long period in the motion of the Moon is an exceedingly laborious and difficult work. Not only are the analytical developments which are involved much more complex than in any other branch of the lunar theory, but they are of very great length. Generally the value of a term of long period is the difference between two large quantities of very nearly equal magnitude. These quantities are usually twenty to thirty times greater than the coefficient to be determined, and are in turn composed of a large number of terms of considerable magnitude and partially destroying each other. Consequently, the neglect of some comparatively insignificant term, or the slightest error of any kind, may be sufficient on one hand to quadruple the true value, or on the other hand to reduce it to zero. The numerical coefficients which are involved in these terms must be carried to a point of extreme accuracy, for I have found the value of an important term to be vitiated from my having carried these numerical coefficients only

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to the fifth decimal place, instead of to the seventh decimal place, although accuracy to the third decimal place appeared to be ample. It is true that these difficulties are present in a very mitigated form in the two terms of long period which were discovered by Hansen. Yet, as the remarks quoted from Hansen show, their determination was found by him to be the most difficult work he had encountered in the lunar theory. Moreover, Poisson and Pontécoulant were unsuccessful in determining the value of one of these two terms, and Delaunay himself failed in his first attempt to calculate the value of the other. All three failures were due to their having omitted some apparently insignificant factor, and so obtaining a value far less than the truth. It was probably on account of the known difficulty of the problem that Delaunay's investigations do not seem to have attracted the attention that was merited by the importance of their subject.

It is now generally admitted that the value which Hansen assigned to the coefficient of the term of long period whose argument he denoted by $(8g''-13g')$ is far greater than its true value, and that we must abandon this second term of long period which is produced by the planet *Venus*. Delaunay's conclusion that its coefficient was very small has been completely confirmed by the entirely independent researches of Professor Newcomb. My own results are in accordance with those of Delaunay, and I have sufficiently verified the analytical developments employed by Delaunay in his memoir to satisfy myself that they are not vitiated by one of those accidental errors from which even the most careful work of the finest mathematicians is not necessarily exempt. It may be regarded as certain, therefore, that Delaunay's investigations are free from any serious error. Yet I understand that Hansen would never admit that Delaunay's researches established the inaccuracy of his own earlier investigations, although I cannot learn that Hansen ever definitely stated the grounds of his objection. It has always appeared to me that Hansen must have had some good reason for his objection, but that he was waiting until he had completed his third investigation of this difficult problem.

My own investigations, whilst they completely confirm the accuracy of Delaunay's result, have led me to the unexpected conclusion that Hansen may be correct after all. Are Delaunay's investigations complete?

I do not refer to the terms depending on the square of the mass of *Venus*, and to which Professor Newcomb has drawn attention in his remarkable paper in the *American Journal of Science* for November 1877, because all investigators have hitherto neglected them, so that it cannot be these terms which produce the discrepancy between Delaunay and Hansen. But I doubt if Delaunay's investigations are really complete as far as terms depending on the first power of the mass of *Venus*, and it appears to me that there is strong evidence that it was from these very

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terms, omitted by Delaunay, that Hansen obtained the large coefficient of the second *Venus* term—that with the argument $(8g''-13g')$.

In the *Memoirs of the Royal Astronomical Society*, vol. xvi., Hansen has published the only existing details of his method of computing these inequalities, and on page 476 there occurs the following passage. Speaking of the term whose argument is denoted by him by $(g-18g''+16g')$, and whose value as far as the first power of the disturbing forces he had just computed, Hansen says: "Cependant le cube des forces perturbatrices, ou plutôt le produit du carré et du cube de la force perturbatrice du soleil par la force perturbatrice de *Venus*, ajoute une quantité sensible à ce coefficient, et rend en même temps sensible le coefficient de l'argument $(8g''-13g')$, qui, en tenant seulement compte de la première puissance de la force perturbatrice, serait insensible dans le mouvement de la lune." Delaunay appears to me to have entirely overlooked the real bearing of this important statement, for I think it clearly defines from whence Hansen obtained the large value for the coefficient of the argument $(8g''-13g')$, which was found to be insensible by Delaunay.

The direct action of the planet *Venus* in producing perturbations in the motion of the Moon may be analytically represented by a particular disturbing function made to form part of the complete disturbing function which represents the action of every disturbing body on the Moon. This particular disturbing function depends on the undisturbed geocentric coordinates of the Moon and *Venus* and on the complete perturbations of the Moon. By neglecting the second and higher powers of the mass of *Venus*, this particular disturbing function is developed in terms of the heliocentric elliptic coordinates of *Venus* and the Earth, the geocentric elliptic coordinates of the Moon, and of the different powers of the perturbations in the motion of the Moon which are produced by the disturbing action of the Sun. Delaunay has exclusively confined himself to this portion of the complete disturbing force when finding the direct action of *Venus* upon the Moon, and, as far as the first power of the disturbing forces, it is evident that this is the only portion which need be considered. As far as the first power of the disturbing forces, Delaunay finds a value for the term $(g-18g''+16g')$ which is sensibly identical with that found by Hansen under like conditions. For the term $(8g''-13g')$ under these conditions Delaunay finds its coefficient is insensible. In this also he agrees with Hansen, as the paragraph which has been quoted will show. To this order Delaunay's investigations must be complete, and to this order both Hansen and Delaunay have obtained identical results.

When Delaunay takes into consideration the effect of the different powers of the perturbations in the motion of the Moon arising from the disturbing action of the Sun, in all cases he considers the effect of the first power of these perturbations, and in one particular term he includes the second power of these pertur-

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bations. Delaunay shows that these terms do not materially augment the value of the two terms of long period. Granting that these results are perfectly accurate, and there can be no real doubt about their accuracy, is there any discrepancy between them and those which Hansen obtained? It cannot be said that there is; for in the paragraph which has been quoted, Hansen explicitly states that the coefficient of the term $(8g''-13g')$ is insensible unless there be included the terms depending on the product of the first power of the disturbing force of *Venus* with the square and cube of the disturbing force of the Sun. But Delaunay has entirely omitted the terms of this order in his investigations. Delaunay himself does not seem to have noticed this discrepancy, partly because he seems to call the first power of the disturbing force of *Venus* also the first power of the disturbing force of the Sun. And his investigations being performed with the aid of his new and elegant method of attacking the lunar theory, in this portion they are not easily followed by anyone not perfectly familiar with this most laborious method. Had this not been so, it is probable that attention would have been directed before now to this discrepancy.

There is little difficulty in ascertaining what were probably Delaunay's reasons for omitting these terms. It is obvious that the terms neglected will be smaller than those which he has retained, and which depend on the first power of the disturbing force of the Sun. But these last are very small, so that there could be no valid reason for including the others which were still smaller. Delaunay was therefore perfectly justified in omitting these terms. But this very circumstance is enough to suggest the inquiry whether Hansen was really referring to these terms, or whether, in his remarks, he does not refer to some entirely distinct class of terms whose existence has been overlooked by Delaunay. Is there, then, a second class of terms to which Hansen's remarks may really apply? If there is, it at once affords a clue to an explanation of the discrepancy between the results obtained by Hansen and Delaunay, for the latter has confined himself entirely to the one particular class of terms which has been described above.

It appears that there is another and still more direct way in which the disturbing action of the Sun will influence the perturbations in the motion of the Moon produced by the disturbing action of a planet.

Let ρ = geocentric distance of the disturbing body; H = geocentric angular distance between the disturbing body and the Moon, r, v = geocentric radius vector and longitude of the Moon; then the disturbing function representing the action of this body upon the Moon will be

$$R_1 = M_1 \left\{ \frac{r^2}{\rho^3} \left(\frac{3}{2} \cos^2 H - \frac{1}{2} \right) + \cdots \right\},$$

where M denotes the mass of the disturbing body. Suppose the

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inclination of the orbit of the Moon and that of the disturbing body to be so small that the product or square of the inclinations may be neglected. Let r_1, v_1 denote the heliocentric radius vector and longitude of the disturbing body, and r_0, v_0 the heliocentric radius vector and longitude of the Earth. Then

$$\rho = \{r_1^2 + r_0^2 - 2r_1 r \cos(v_0 - v_1)\}^{\frac{1}{2}},$$

$$\cos H = \left\{\frac{r}{\rho}\cos(v - v_1) - \frac{r_0'}{\rho}\cos(v - v_0)\right\}.$$

Substituting this value of cos H in the expression for R_1, it becomes

$$R_1 = M_1\left\{\frac{1}{4}\cdot\frac{r^2}{\rho^3} + \frac{3}{4}\frac{r^2}{\rho^5}[r_1^2\cos(2v - 2v_1) + r_0^2\cos(2v - 2v_0) - 2r_1 r_0\cos(2v - v_1 - v_0)]\right\}.$$

This will be the form of the disturbing function which expresses the direct action of a planet upon the Moon, and it is to this portion of the complete disturbing function that Delaunay has entirely confined his attention. The disturbing function which expresses the disturbing action of the Sun upon the Moon will be found by making $r_1 = 0$ in the above. It will be

$$R_0 = M_0\left\{\frac{1}{4}\cdot\frac{r^2}{r_0^3} + \frac{3}{4}\frac{r^2}{r_0^3}\cos(2v - 2v_0)\right\}.$$

Let the complete disturbing function be denoted by R, and suppose it to be developed in a series of cosines in the usual manner,

$$R = R_0 + R_1 = \Sigma R_0 \cos\beta_0 + \Sigma R_1 \cos\beta_1 = \Sigma R \cos\beta.$$

Throughout the subscript $_0$ will be employed to denote quantities arising from the disturbing action of the Sun, and the subscript $_1$ to denote all quantities arising directly or indirectly from the disturbing action of the planet. Let R^o denote the value of R when the perturbations of the coordinates are neglected; it is thus necessary to develop R in terms of R^o and the perturbations. Both R_0 and R_1 involve the lunar coordinates in identically the same manner, so that their differentials with respect to these coordinates will possess the same identity. Direct differentiation gives

$$\left(\frac{1}{r}\right)^n \frac{d^n R_1^o}{d\left(\frac{1}{r}\right)^n} = (-1)^n \cdot \Gamma(n+2) \cdot \Sigma R^o \cos\beta,$$

$$\frac{d^n R}{dv^n} = 2^n \cdot \cos n\pi \cdot \Sigma R^o \cos\beta - 2^n \cdot \sin n\pi \cdot \Sigma R^o \sin\beta.$$

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The differential with regard to v vanishes for those terms in the disturbing function not arising from the part of the disturbing function involving v, that is for all terms whose arguments are independent of the angular distance of the Moon and the Sun or other disturbing body. For the present purpose there is no need to separate these two portions, as the distinction can be remembered.

Suppose the complete perturbations of the Moon to be expressed by

$$r\delta\frac{1}{r} = \Sigma a_0 \cos \alpha_0 + \Sigma a_1 \cos \alpha_1, \qquad \delta v = \Sigma b_0 \sin \alpha_0 + \Sigma b_1 \sin \alpha_1.$$

To develop R let

$$D^0_r = \left(r\delta_0 \frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right), \qquad D^1_r = \left(r\delta_1 \frac{1}{r}\right)\left(\frac{1}{r}\cdot\frac{d}{d\frac{1}{r}}\right),$$

$$D^0_v = (\delta_0 v)\left(\frac{d}{dv}\right), \qquad D^1_v = (\delta_1 v)\left(\frac{d}{dv}\right),$$

then a known symbolical expansion gives

$$\mathrm{R} = \mathrm{R}^0 + \delta\mathrm{R}$$

$$= \{^{D^0_r + D^1_r + D^0_v + D^1_v}\}\mathrm{R}^0$$

$$= \{e^{D_r}.e^{D^1_v}\}\{e^{D^0_r}.e^{D^0_v}\}\mathrm{R}^0$$

$$= \{(1 + D^1_r + \cdots)(1 + D^1_v + \cdots)\} \times$$
$$\{(1 + D^0_r + \tfrac{1}{\lfloor 2}(D^0_r)^2 + \cdots)(1 + D^0_v + \tfrac{1}{\lfloor 2}(D^1_v)^2 + \cdots)\}\mathrm{R}^0.$$

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Replace D and its powers and products by their values and perform the operations, then the expression for R becomes

$$R = \Sigma R^{\circ}\{[\cos\beta - 2(a_{\circ}\cos\alpha_{\circ})\cos\beta - 2(b_{\circ}\sin\alpha_{\circ})\sin\beta + 3(a_{\circ}\cos\alpha_{\circ})^{2}\cos\beta + 4(a_{\circ}\cos\alpha_{\circ})(b_{\circ}\sin\alpha_{\circ})\sin\beta - 2(b_{\circ}\sin\alpha_{\circ})^{2}\cos\beta + \cdots = \overset{\pm}{\Sigma}A_{\circ}\cos(\beta \pm \alpha_{\circ})]\}$$

$$+ \Sigma R^{\circ}\{[\cos\beta - 3(a_{\circ}\cos\alpha_{\circ})\cos\beta - 2(b_{\circ}\sin\alpha_{\circ})\sin\beta + 6(a_{\circ}\cos\alpha_{\circ})^{2}\cos\beta + 6(a_{\circ}\cos\alpha_{\circ})(b_{\circ}\sin\alpha_{\circ})\sin\beta - 2(b_{\circ}\sin\alpha_{\circ})^{2}\cos\beta + \cdots = \overset{\pm}{\Sigma}B_{\circ}\cos(\beta \pm \alpha_{\circ})] \times [-2a_{1}\cos\alpha_{1}]\}$$

$$+ \Sigma R^{\circ}\{[\sin\beta - 2(a_{\circ}\cos\alpha_{\circ})\sin\beta + 2(b_{\circ}\sin\alpha_{\circ})\cos\beta + 3(a_{\circ}\cos\alpha_{\circ})^{2}\sin\beta - 4(a_{\circ}\cos\alpha_{\circ})(b_{\circ}\sin\alpha_{\circ})\cos\beta - 2(b_{\circ}\sin\alpha_{\circ})^{2}\sin\beta + \cdots = \overset{\pm}{\Sigma}A_{\circ}\cos(\beta \pm \alpha_{\circ})] \times [-2b_{1}\sin\alpha_{1}]\}$$

$$+ \cdots$$

Let $a_{\circ}a_{\circ}b_{\circ}$ denote the coefficient of the argument $\alpha_{\circ}$ in the function $(a_{\circ}\cos\alpha_{\circ})^{2}(b_{\circ}\sin\alpha_{\circ})$ and similarly with other terms; then

$$\overset{\pm}{\Sigma}A_{\circ} = 1 - a_{\circ} \pm b_{\circ} + \frac{3}{2}a_{\circ}a_{\circ} \mp 2a_{\circ}b_{\circ} - b_{\circ}b_{\circ} + \cdots \qquad \overset{\pm}{\Sigma}B_{\circ} = 1 - \frac{3}{2}a_{\circ} \pm b_{\circ} + 3a_{\circ}a_{\circ} \mp 3a_{\circ}b_{\circ} - b_{\circ}b_{\circ} + \cdots$$

and the disturbing function can be written

$$R = \Sigma R'_{1}(\overset{\pm}{\Sigma}A_{\circ})\cos(\beta_{1} \pm \alpha_{\circ}) + \Sigma R^{\circ}_{\circ}\{(\overset{\pm}{\Sigma}A_{\circ})\cos(\beta_{\circ} \pm \alpha_{\circ}) + [\overset{\pm}{\Sigma}B_{\circ}\cos(\beta_{\circ} \pm \alpha_{\circ})](-2a_{1}\cos\alpha_{1}) + [\overset{\pm}{\Sigma}A_{\circ}\sin(\beta_{\circ} \pm \alpha_{\circ})](-2b_{1}\sin\alpha_{1}) + \cdots\}.$$

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In these expressions all the upper and all the lower signs are to be taken together, and the terms depending on the square of the mass of the disturbing planet have not been written down. There is obviously not the slightest difficulty in at once writing down every term to any desired order of the disturbing forces. When β is an argument which is independent of the angular distances between the Moon and disturbing body, the corresponding coefficient b must be put equal to zero in the coefficients ΣA_0 and ΣB_0. This is the form of the disturbing function which I have employed in my investigations on the terms of long period in the motion of the Moon; and it is particularly convenient, from the portions depending on the solar perturbations being kept distinct from that depending on the disturbing planet, so that they will be identical for every term of this kind in the theory of the Moon.

The first portion of this expression is the development of the disturbing function which has been denoted by R_1, and which represents the direct action of the planet upon the motion of the Moon, and upon the perturbations in this motion arising from the disturbing action of the Sun. It is this portion alone of the above expression which has been considered by Delaunay. The first term in the second portion is the expression of the part of the disturbing function due to the disturbing action of the Sun, and from which are derived the ordinary perturbations of the lunar theory. The second and third terms of this second part arise from substituting the complete perturbations of the Moon in the value of the ordinary disturbing function representing the action of the Sun upon the motion of the Moon. They really represent the portion of the disturbing function which expresses the action of the Sun upon the perturbations produced by the disturbing action of the planet upon the motion of the Moon. They yield the new class of terms to which attention has been directed.

The theory of the variation of parameters gives the following equation for determining the terms of long period in the mean motion of the Moon:—

$$\frac{d^2\delta n}{dt^2} = -3an^2\left(\frac{dR}{dE}\right),$$

where a is the mean distance, and E the longitude of the epoch. Let f be the arithmetical coefficient of E in the argument which has been denoted by β; then

$$\left(\frac{dR}{dE}\right) = \Sigma(-f)\,R\sin\beta.$$

The complete development of this expression can be obtained from that of R by writing $\beta-\frac{\pi}{2}$ for the arbitrary argument β. It will be therefore

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$$\left(\frac{dR}{dE}\right) = \Sigma(-f).R^{\circ}_{1}\,(\overset{\pm}{\Sigma}A_{\circ})\sin(\beta_{1}\pm\alpha_{\circ}) + \Sigma(-f)\,R^{\circ}_{\circ}\{(\overset{\pm}{\Sigma}A_{\circ})\sin(\beta_{\circ}\pm\alpha_{\circ})$$

$$+ [\overset{\pm}{\Sigma}B_{\circ}\sin(\beta_{\circ}\pm\alpha_{\circ})](-2a_{1}\cos\alpha_{1}) + [\overset{\pm}{\Sigma}A_{\circ}\cos(\beta_{\circ}\pm\alpha_{\circ})](-2b_{1}\sin\alpha_{1}) + \cdots\}$$

It remains to be shown that the second portion of this expression will give rise to terms of long period with the same argument as those which can be obtained from the first portion, and which have alone been considered by Delaunay.

Let $Z\cos\zeta$ denote the complete value in $r\delta\,\frac{1}{r}$ of the term of long period which is derived from the first part of the above expression, and $Z^{1}\sin\zeta$ its complete value in δv. These will then form particular forms of the general terms $a_{1}\cos\alpha_{1}$ and $b_{1}\sin\alpha_{1}$. The function which forms the coefficient of the term $(-2b_{1}\sin\alpha_{1})$ will have a constant portion of the order of the square and cube of the disturbing force of the Sun, and consisting of the coefficient of every pair of arguments in the series of arguments expressed by $\beta_{\circ}$ and $\alpha_{\circ}$ which make $\beta_{\circ} = \alpha_{\circ}$.

This constant portion will be of the order m^{4}, and may be denoted by the term $\Sigma(R_{\circ}^{\circ})_{x}(\Sigma A_{\circ})_{x}$. Substituting these two values, then, as far as the term with the argument ζ, there will exist the term

$$\left(\frac{dR}{dE}\right) = \{\Sigma(R^{\circ}_{\circ})_{x}\cdot(\Sigma A_{\circ})_{x}\}(-2Z^{1}).\sin\zeta.$$

In a similar manner other terms will arise from this second part of the expression having the argument ζ and with coefficients containing Z and Z^{1} multiplied by quantities of the order of the square and cube of the disturbing force of the Sun. Now Z and Z^{1} are of the order of the disturbing force of the planet. This new class of terms must be described, therefore, as arising from the product of the square and cube of the disturbing force of the Sun with the disturbing force of the planet.

I think it is unquestionable that it is to these terms that Hansen refers in the paragraph which I have quoted from his memoir, and that it is from these terms that Hansen has derived the large value of the coefficient of the term of long period with the argument $(8g'' - 13g')$. For it is to these terms, and to these terms alone, that Hansen's words naturally apply. As these terms have been entirely neglected by Delaunay, this view would at once afford a possible explanation of the discrepancy between Hansen and Delaunay, and show at once why Hansen would not admit the correctness of the conclusions which were obtained by Delaunay. For these terms, depending on the ordinary perturbations arising from the direct action of *Venus* multiplied by the second power of the disturbing force of the Sun, will appear in the perturbations of the mean motion divided by the square of the very small coefficient of the time. They can very easily be far greater than the perturbations arising from

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the direct action of *Venus*, which have been alone considered by Delaunay (the indirect effect of *Venus* through the agency of the perturbations of the Earth being disregarded). I think, therefore, it is almost certain that it is in this way that the discrepancy between Hansen and Delaunay has arisen.

It does not necessarily follow that the theoretical results which were obtained by Hansen are necessarily correct, although I have no doubt that it was from this second class of terms that Hansen derived the large value of the coefficient of the doubtful *Venus* term, and that consequently Delaunay's investigations do not really invalidate Hansen's conclusions. From his own remarks, it is evident that Hansen found very great difficulty in determining the theoretical value of these terms, a work of immense labour and difficulty by any of the ordinary methods. It is very possible that both of his results may have been vitiated by some error, just as one of the two conflicting results must have been wrong. The computations being extremely long, it is not unlikely that some error may have crept in; and it must be remembered that Delaunay, by neglecting these terms, has omitted the most difficult portion of the computation. It may be that these terms are altogether insensible. It is still more probable that the largest portion of them are mutually destructive, and in this manner vanish. I would urge merely that, until they have been shown to be insensible or to disappear, the theory of these two terms of long period must be considered to be imperfect.

These terms are of the third and fourth order of the disturbing forces, and it may be that it is possible to show by some established analytical principle, such as the invariability of the semi-axis major, that these terms must be mutually destructive. It is possible that Professor Newcomb may have investigated this point in his unpublished researches, and have thus shown that these terms do not affect the terms of long period in the lunar theory. I should be glad to learn whether this is so, for the past history of the lunar theory shows more than one instance where such principles have been overlooked, or else erroneously imagined to exist. To be sure that these terms may be safely neglected when determining the value of the coefficients of terms of long period similar to Hansen's, it must be shown that they vanish as far as the order m^7. To effect this by a direct computation would be a difficult and lengthy task. To show that they may be neglected in the case of all the terms of long period to which I have directed attention, it would be necessary to prove that they vanish to the order m^{10}. To effect this by direct computation would be an enormous work, and one which could not be carried out without pushing the lunar theory to a higher order than has ever yet been done.

I have endeavoured, therefore, to determine the value of these terms by a different method. In many portions of the lunar theory there arise different classes of important terms which in

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the end destroy each other and so vanish. By developing the disturbing function in a general form it is possible, by an application of the method of the variation of arbitrary constants, to ascertain what terms destroy each other without its being necessary to determine the value of the terms in question. Thus if R be a function of the form $r \begin{matrix}\sin\\ \cos\end{matrix} \beta$, where r is constant and only β variable, as far as constant terms the function

$$R \int R\, dt = 0.$$

It can be further shown that in this expression every term is multiplied by the coefficient of the time in its argument. By similar considerations to these the work can be much reduced.

To show that the class of terms which have been referred to vanish to the order m^{10}, it would be necessary to show that they are mutually destructive to the fifth power of the disturbing forces, whereas no general development of the theory of the variation of constants seems to have been pushed beyond the terms of the second power of the disturbing forces. By a transformation I have pushed the development as far as the third power of the disturbing forces, and in part, though at present not completely, to the fourth power.

By this means it can be shown that the terms of the order m^4 and m^5 vanish from the perturbations in the mean motion, or before integration are multiplied by the small coefficient of the time in their argument. The same is true of most if not all of the terms arising from the second power of the disturbing force of the Sun and of the order m^6 and upwards. The same does not appear to hold of the third power of the disturbing force of the Sun or of the fourth power of the entire disturbing forces. I have not yet completed this last portion, but terms of the order m^6 appear to actually remain undestroyed. This result is confirmatory of the results obtained by direct computation in a very different manner.

If, then, there remain terms of the order m^6, it is quite possible that Hansen's theoretical values of the two terms of long period arising from *Venus* may be accurate.

On the Telescopic Observations of the Transit of Venus 1874, *made in the Expedition of the British Government, and on the Conclusions to be deduced from those Observations.* By E. T. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

I received by the last mail a copy of the Parliamentary Report whose title heads this Paper. This report contains the details of the observations of contact made by the observers of the British

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Expeditions and the deduction of a value of the mean solar parallax (8″·76). I have examined the observations with great care, and discussed them in accordance with my own views. The result to which I have been led differs most materially from the value 8″·76 obtained in the Report, and I have therefore felt it my duty to lay my views before astronomers for their consideration. I presume that, with the evidence of these British observations, as well as that of the French observations before us, we need no longer insist upon the fact that the contacts in a transit of *Venus* are not mere geometrical contacts, but that when well seen in a fairly illuminated field, and when attention is directed to the point, the connection between the apparent limbs of *Venus* and the Sun is formed and broken gradually. The exact name which different observers may please to give to the phenomena observed matters but little, provided we can identify the observations which have taken place with the same angular separation of the limbs. The choice of assumed difference of semi-diameters applicable to the observed contacts is, as I have always maintained, the one difficulty in obtaining a very accurate value of the solar parallax from discussions of contact observations of the limbs of *Venus* and the Sun, and this same difficulty is presented in the methods of Halley and Delisle. The whole strength of the method for the determination of the solar parallax rests upon the assumption that there are phases of the contact which can be recognised by the observers as distinctive, and which at all the stations take place with the same angular separation of the limbs. It is not of course assumed that this is absolutely true; but the accuracy with which this assumption can be made in any class of observations, under the actual circumstances of observation, combined with the magnitudes of the parallax-factors, alone limits the accuracy attainable in the deduced value of the solar parallax. It is useless, therefore, except as a check on the principal phase, to record times corresponding to any but well-marked phases; and all time records corresponding to "limbs well separated," "bright light between the limbs," and such like, except as an indication of the time of last appearance of some disturbance previously seen, are without value for our present purpose.

I see no reason for abandoning my old views that the two phases of greatest value are the so-called apparent contacts and the so-called real contacts, of which the latter is the more important. By the real contact I merely mean the phase at which the observer first perceives the formation or disappearance of any permanent connection between the limbs of *Venus* and the Sun.

When we come to examine the observations made in 1874 we find that the apparent contacts have not been observed at the Sandwich Islands, one of the principal stations. It would almost appear from Mr. Nichol's remarks, and those of Capt. Tupman, that the measures of the cusps were run too close to the time of

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contact to allow of any successful attempt to observe the apparent contacts. I am therefore compelled to restrict myself entirely in the present Paper to the real contact; but I shall divide them into two classes: the first corresponding to a disturbance of a very slight character, which can only, in my opinion, be seen under the conditions of a bright field and fair definition; and the second and more important phase when this shadow, or black drop, or haze, first assumes, or last appears of, a well-marked and permanent character. It will be seen, from the few attempts at the first phase, that the change of distance of centres is sufficiently great to prevent any confusion arising between these two kinds of observations, and the principal phase is well separated from the apparent contacts.

Sandwich Islands.

The following is Mr. Johnson's account of his observation at Waimea:—

	h	m	s	
{Cusps / Indefinite light} beginning to appear to meet .	7	17	57·8	G.S.T.
Meeting of cusps appeared to take place, and light clearing. I am inclined to think the actual contact took place between $7^h\ 17^m\ 57^s{\cdot}8$ and $7^h\ 18^m\ 7^s{\cdot}1$	7	18	7·1	
A distinct band of light between edge of *Venus* and the Sun's limb	7	18	12·0	

I have taken this observation under consideration, first, because it is preceded and followed by remarks showing that the time of total disappearance of the connection could not differ much from the time recorded, viz. $7^h\ 18^m\ 7^s{\cdot}1$. It is, however, probable that a mean between the two observations, $7^h\ 17^m\ 57^s{\cdot}8$ and $7^h\ 18^m\ 7^s{\cdot}1$, might be the true time of disappearance of the ligament.

Next, referring to the Honolulu observations, we have Capt. Tupman's observation:—

	h	m	s	
There was no *black* drop nor ligament. The planet was perfectly circular, and nothing whatever disturbed the sharpness of its outline at the place of contact. The band of light gradually brightened, and as Mr. Clapp said "twenty" it was sufficiently bright to induce me to consider the contact as established. My impression *afterwards* was that even at this time the Sun's limb was not yet perfectly distinct, and that some shadow remained on the light, but nothing was recorded after the "twenty." At that time the general appearance was similar to the model a second or two after contact	7	17	59·8	G.S.T.

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Lieut. Noble's observation:—

	h	m	s	
There was no black drop, no ligament, but a rough dark shade, which gradually faded off to a thin tint corresponding to the phenomena I had observed in the model. This, instead of being nearly instantaneous, as the model generally showed, extended over some 20 seconds. The thin tint is that recorded, and I consider this the time of contact. I am certain it could not have been later than this	7	18	0·2	G.S.T.
Capt. Cator's observation (with diagram)	7	18	2·2	

Lieut. Noble also states that at $7^h\ 18^m\ 20^s{\cdot}2$ the contact was long passed.

Now, we have here four observations of the real contacts. They differ amongst themselves rather more than could be wished, but not more, I believe, than such observations must fairly be expected to differ. There would be a better appearance of agreement if we took for Johnson's time the mean as before mentioned; but it appears to me better to take the results as they stand, and we have for real contact at the Sandwich Islands—

	Greenwich Sidereal Time. h	m	s	Local Tabular Distance of Centres. ′	″
Capt. Tupman	7	17	59·8	15	37·58
Lieut. Noble	7	18	0·2	15	37·57
Mr. Johnson	7	18	7·1	15	37·35
Capt. Cator	7	18	2·2	15	37·40
Mean	7	18	2·3	15	37·48

Then, if x = a constant for Ingress observations of real contact, $\delta\pi$ = connection to the assumed parallax 8″·95, and $\delta\pi = 8{\cdot}95 \times \frac{y}{100}$, we have for the real contact equation

$$x + 0{\cdot}2280\,y = 15'\ 37''{\cdot}48 \qquad 4 \text{ observations.}$$

It will be seen that the discordance in the estimation of the contact does not differ for any observer more than 0″·13 from the mean, a small quantity to be measured on the Sun's edge.

Rodriguez.

The real contact observations made by Commander Wharton and Lieut. Neate at this station are both clearly marked as the time of last appearance of haze or ligament.

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Commander Wharton says :—

	h	m	s	
Directly after, a blackish haze was discernible joining the planet and the continuation of the Sun's limb; but from the agitation of the limb (the morning being cloudy) this haze was not distinctly defined and did not seem to change colour before disappearance. The moment of total disappearance of haze was tolerably defined, but I could not be certain to two or three seconds. The band of haze did not narrow at all before it broke that I could see. The time recorded is of total disappearance of haze as nearly as could be guessed	7	39	31·0	G.S.T.

Lieut. Neate :—

The ligament then gradually changed in colour (but not in breadth) getting lighter till it disappeared apparently suddenly, when the light between the limbs was uninterrupted . .	7	39	27·2	

Lieut. Hoggan :—

The ligament appeared to be decreasing in width very perceptibly	7	39	33·1	
It had faded away, and light appeared all round the planet's disk	7	39	48·6	

The first recorded time is early, the last would appear, from the use of the past tense, too late. A mean time, or $7^h\,39^m\,41^s{\cdot}85$, cannot be much in error and appears preferable to the use of either of these times. I may mention that, had I used the earlier time as that of last appearance, it would have given me a very slightly larger value of the solar parallax. Thus we have for real contacts at this station :—

	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Com. Wharton	7	39	31·0	15	37·88
Lieut. Neate	7	39	27·2	15	38·00
Lieut. Hoggan	7	39	41·9	15	37·58
Mean	7	39	33·4	15	37·82

Real contact equation $x - 0{\cdot}1947\,y = 15'\,37''{\cdot}82$ 3 obs.

At Kerguelen we have observations of the disappearance of the connection or real contacts by three observers.

Lieut. Corbet :—

	h	m	s	
At this moment there was no more shade between the two limbs; it disappeared	7	40	33·2	G.S.T.

Lieut. Coke :—

The film disappeared	7	40	49·6	

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Lieut. Goodridge:—

	h	m	s	
All appearance of shadow totally disappeared .	7	40	43·6	G.S.T.

Lieut. Corbet's time appears decidedly too early; but, on account of the difficulty of such observations, it does not appear desirable to assume too small a probable error, and I take the mean. It will be seen on consideration that if I had given less weight to Lieut. Corbet's observation, the value of the solar parallax deduced would have been greater. We have, then, for this station:—

	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Lieut. Corbet	7	40	33·2	15	38·29
Lieut. Coke	7	40	49·6	15	37·81
Lieut. Goodridge	7	40	43·6	15	37·93
Mean	7	40	42·1	15	38·01

and the equation of real contacts is

$$x - 0{\cdot}2150\,y = 15'\,38''{\cdot}01. \qquad 3 \text{ obs.}$$

There remains the ingress observed by Major Palmer at Burnham. He says:—

	h	m	s	
At . . . . . as far as could be judged, the ligament seemed to undergo a change in depth of colour; but clouds prevented me from seeing whether any streak of light connecting cusps played across it.	7	29	38·1	G.S.T.
My last sight of the planet was at . . . . at which moment I *thought* I saw a very slight wavering streak of light, *though the cusps* (now very close) *had not fairly met.*	7	29	41·3	
I judged that it wanted about five seconds to complete separation of limbs. I counted the clock ticks for five seconds, and gave a signal for an estimated contact at	7	29	46·3	
The Sun did not appear again until about . .	7	43		

On account of cloud the absolute disappearance of the ligament at this station is only an estimation, but it was not gone at $7^h\ 29^m\ 41^s{\cdot}3$, which is important. Major Palmer's estimate places its disappearance at 5^s later, but such an estimation made during a glimpse through cloud could only be a rough one. I wish, however, to show that I am not straining the evidence in favour of any particular value of the solar parallax, and I will accept it as an observation, and work up the Ingress observations both including and excluding it.

The real contact equation derived from this observation is

$$x - 0{\cdot}0140\,y = 15'\,38''{\cdot}07.$$

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It will be seen that, on account of the small factor of y, this observation is not of much value for the determination of the solar parallax.

First, excluding the Burnham observation, the three equations for Ingress are

$$\sqrt{4}\,(x+0\cdot2280\,y) = \sqrt{4}\times0\cdot48,$$
$$\sqrt{3}\,(x-0\cdot1940\,y) = \sqrt{3}\times0\cdot82,$$
$$\sqrt{3}\,(x-0\cdot2150\,y) = \sqrt{3}\times1\cdot01,$$

which lead to

$$x-0\cdot0315\,y = +0\cdot741,$$
$$x-1\cdot4586\,y = +2\cdot193;$$

whence

$$x = +0''\cdot71,\ y = -1\cdot017,\ \text{and solar parallax} = 8''\cdot859.$$

These values give the following residual errors :—

Obs.—Comp.	
$+0''\cdot00$	Sandwich Islands.
$-0\cdot09$	Rodriguez.
$+0\cdot08$	Kerguelen.

Next, including the Burnham observation, we have, to find x and y, the equations

$$x-0\cdot0299\,y = +0\cdot771,$$
$$x-1\cdot3971\,y = +2\cdot145;$$

whence

$$x = +0''\cdot741,\ y = -1\cdot005,\ \text{and solar parallax} = 8''\cdot860.$$

The residual errors are now

$-0''\cdot03$	Sandwich Islands.
$+0\cdot31$	Burnham.
$-0\cdot12$	Rodriguez.
$+0\cdot05$	Kerguelen.

It would appear that Major Palmer's estimation is about 10^s too early, a result which can surprise no one. The general result from the Ingress observations is therefore

$$\pi = 8''\cdot860 \pm 0\cdot136\times e,$$

where e is the probable error estimated in arc between the difference of the tabular distance of the centres corresponding to the time given by an observer and that which corresponds to the mean phase adopted at all the stations for Ingress. I

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think e is certainly not greater than 0″·3, and that the probable error of the above result cannot exceed 0″·04.

Anyone who will examine my work will find that I have not selected the observations with a view to securing the largest possible value of the solar parallax. I have boldly taken the mean results of all attempts to observe the same phase, and I have done this on principle, although the observations may appear to differ somewhat wildly amongst themselves. For instance, I have taken for the time of the last appearance of the ligament at Kerguelen's Island the mean of the three results by Corbet, Coke, and Goodridge, although Corbet's time is 16^s earlier than that given by Coke; but in this transit of 1874 the velocity of separation of the limbs was so small that an error of 16^s is equivalent to less than an error of half a second of arc in the separation of the limbs, and this is certainly not an impossible quantity for the disagreement of two measures made upon the Sun's limb. As we have, however, the positive evidence of two observers that some connection between the limbs did exist for more than 10^s after the time given by Corbet, we might no doubt legitimately assume that all connection between the limbs of *Venus* and the Sun had not ceased at the time indicated by Corbet; but in my opinion we could not therefore legitimately reject his observation. It must be remembered that when we attempt to draw a value of the solar parallax from contact observations, we have to make an assumption that all attempts to observe the same phase are made with the same angular separation of the limbs; and it is certainly more probable that such an assumption will be satisfied when mean values are adopted rather than extreme ones. The discordances which exist amongst the observers at the same station are independent of any assumed value of parallax, and are therefore valuable as an indication of the errors which may be expected when we assume that the contacts for all stations take place with a mean distance between the centres; and we cannot fairly expect the discordances which arise from this assumption to be quite as small as those which exist among observations of the same phase made at a single station. I have been guided by this consideration in the rather liberal allowance which I have made for the probable error of the result.

The only cases in which I consider that I have exercised any arbitrary selection of the materials are:—

(1) The above case of Corbet (I might have given the observation less weight, or rejected it);
(2) The case of Johnson (I might have taken the mean value as before indicated);
(3) The case of Hoggan (in which I have taken a mean value).

But none of the alternatives to the selection which I have adopted can change the value of the solar parallax more than

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0″·02, and I believe that I have had good reasons for the selections made.

It will of course be easy for anyone to vary the solution within small limits by slight changes such as those indicated, but I believe that no important changes in the value 8″·86 is possible without some clearly erroneous selection of a phase attempted to be caught by the observer. In the original solution contained in the Report an assumption has been made that what I have taken to be real contacts at the Sandwich Islands are some phase sensibly preceding the total disappearance of the connection between the limbs of *Venus* and the Sun; whilst some remarks made by the observers at different intervals after the total disappearance of any connection are supposed to be the real contact observations, and are marked "phase γ." These remarks, I have no doubt, were merely made by the observers to check the reality of the disappearance of any connection; and as they have no distinct relation to a definite separation of the limbs, they are only useful as checks upon the principal phase. It is this mistake which is the principal and almost sole cause of the difference between the value of the solar parallax obtained in the Parliamentary Report and that which I have obtained from a discussion of these Ingress observations. Before passing to the Egress observations, I would remark that the observations at Ingress and Egress are of a somewhat different character. At Ingress the observers are carefully watching the disappearance of something distinctly seen; whilst at egress they have to pick up, sometimes even through clouds, the first clear indications of a disturbance of the Sun's limb near the point of contact. There is therefore no great certainty, nor is there any necessity, that the phases at Ingress and Egress should exactly correspond. I fear, moreover, that attempts to estimate the degree of blackness of the ligament at Egress have led some of the observers to wait rather late before they have considered the contact as fairly established, and I doubt the expediency of these attempts to subdivide the phases.

The value of the Egress observations contained in the Parliamentary Report is also much diminished through the comparative failure, on account of cloud, of the observations at Kerguelen's Island; but the observations clearly point to a larger value of the solar parallax than that which I have deduced from the Ingress observations.

Mokattam and Suez.

We have four observations of the ligament well marked:—
Captain Browne says:—

> The shadow began to appear, and I tried to note the instant, but feared to interrupt my wife counting so near contact. Tried to note some

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	h	m	s	
one instant, but could not, *the shadow was so gradual*. The Sun getting brighter, I saw a modified short ligament form, but did not see the edges form into sharp curves and creep apart, as sometimes in the model. Blackness gathered gradually, and at last came an instant when it seemed to get as black as the planet, and to have no light crossing it, which I noted by repeating the seconds then called by my wife . . .	11	17	17·8	G.S.T

Mr. Hunter :—

Ligament now well marked	11	17	24·08	

At this station, then, we have the following observations :—

	Greenwich Sidereal Times. h	m	s	Local Tabular Dist. of Centres. ′	″
Mr. Newton	11	17	14·3	15	41·34
Miss Newton	11	17	16·6	15	41·41
Capt. Browne	11	17	17·8	15	41·45
Mr. Hunter	11	17	24·1	15	41·62
Mean	11	17	18·2	15	41·455

The equation for this station for *ligament well marked* is

$$x + 0{\cdot}2020\,y = 15'\,41''{\cdot}455 \qquad 4 \text{ obs.}$$

There is also a very early observation of a fine line at $11^h\ 16^m\ 39^s{\cdot}1$ with a tabular distance of centres equal to $15'\ 40''{\cdot}08$, and two geometrical contacts which correspond to a tabular distance $15'\ 44''{\cdot}54$ of the centres. It will be seen that the principal phase is quite separated from the two other phases, and that no confusion need be feared from the use of one for the other, except for the observations at Kerguelen, which were made through clouds. The observers, however, appear to have waited until the ligament was very strongly developed.

Thebes.

There is a little difficulty in interpreting the observations at this station, from the language of the observers; but the following three so-called "contacts" must be the same phase as that observed at Mokattam and Suez. This appears not only from the value of the tabular distances of centres, but also from the kind of contact represented in Colonel Campbell's figure :—

	Greenwich Sidereal Times. h	m	s	Local Tabular Dist. of Centres. ′	″
Admiral Ommaney	11	16	46·8	15	41·43
Mrs. Campbell	11	16	51·0	15	41·56
Col. Campbell	11	16	53·3	15	41·64

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There is an early observation of Mrs. Campbell's at $11^h\ 15^m\ 44^s{\cdot}3$, corresponding to a tabular distance $15'\ 39''{\cdot}31$ and called an appearance of shadow, and also one at $11^h\ 16^m\ 29^s{\cdot}5$, with a tabular distance $15'\ 40''{\cdot}84$ and called a small black drop. The equation for this station for ligament very well marked is

$$x + 0{\cdot}1910\,y = 15'\ 41''{\cdot}543 \qquad 3 \text{ obs.}$$

Rodriguez.

We have here two times recorded near the real contact so close together that some doubt may exist as to which should be taken. I will exhibit the results on both suppositions, and it will be found that the resulting values of the solar parallax do not sensibly differ.

Lieut. Neate:—

	h	m	s	
The first phenomenon noticed was the formation of a brown haze between the two limbs, and in spite of the excessive boiling this formation appeared instantaneously, and in appearance it closely resembled what I had seen at ingress in the second noted phenomenon	11	8	39·7	G.S.T.

Lieut. Hoggan:—

The light was not entirely interrupted at . .	11	8	41·4	
Light no longer visible between the limbs of the Sun and *Venus*, being interrupted by a band of about one-eighth of the diameter, and the same degree of blackness as the planet	11	8	46·4	

Commander Wharton:—

The brown haze appeared quickly, though not well defined at its edges as a ligament; it was of considerable breadth. Time recorded is of first appearance	11	8	41·7	
The haze or ligament darkened perceptibly. It is recorded as first appearance of a black hazy drop, the time of which was not very distinctly marked	11	8	50·3	

There are also observations of a very early phase by Lieut. Hoggan, at $11^h\ 8^m\ 8^s{\cdot}5$ G.S.T., of a slight distortion of the planet's limb which did not remain steady, corresponding to a tabular distance $15'\ 40''{\cdot}30$, and three apparent contacts corresponding to a mean distance $15'\ 43''{\cdot}307$. Our observations are then:—

1st Set.	Greenwich Sidereal Time. h	m	s	Local Tabular Dist. of Centres. ′	″
Lieut. Neate	11	8	39·7	15	41·27
Lieut. Hoggan	11	8	41·4	15	41·37
Comm. Wharton	11	8	41·7	15	41·34
Mean				15	41·327

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and the equation for this phase is

$$x + 0{\cdot}0270\,y = 15'\ 41''{\cdot}327 \qquad 3 \text{ obs.}$$

2nd Set.	h	m	s	′	″
Lieut. Hoggan	11	8	46·4	15	41·53
Com. Wharton	11	8	50·3	15	41·62
Mean				15	41·575

and the equation for ligament strongly marked is

$$x + 0{\cdot}0270\,y = 15'\ 41''{\cdot}575 \qquad 2 \text{ obs.}$$

Kerguelen's Island.

Rev. W. Sidgreaves:—

	h	m	s	
Sun nearly obscured. Circular contact. Nothing seen before this. Very uncertain observation; image scarcely visible through the cloud . .	11	1	58·1	G.S.T.

Rev. S. J. Perry:—

A good geometrical contact. There was no appearance of black drop before contact, but this might have been due to the obscuring clouds .	11	2	15·8	

Lieut. Goodridge:—

Passing clouds. At this time a few lines or slight shadow appeared between the two limbs, just visible through cloud	11	1	39·9	
At this time the shadow gradually darkened until the black drop assumed a solid appearance of the same density as the planet. A dark cloud came over, and I lost all further view . .	11	1	53·4	

We have, then, the following observations at this station:—

	Greenwich Sidereal Times. h	m	s	Local Tabular Dist. of Centres. ′	″	
Lieut. Goodridge	11	1	39·9	15	40·61	Shadow.
Lieut. Goodridge	11	1	53·4	15	41·03	Solid black drop.
Rev. W. Sidgreaves	11	1	58·1	15	41·14	Contact.
Rev. S. J. Perry	11	2	15·8	15	41·68	Contact.

Whatever interpretation we may be pleased to put upon these observations, there is no doubt that a well-marked connection was established at $11^h\ 1^m\ 55^s{\cdot}7$ G.S.T. This is shown by both Sidgreave's and Goodridge's observations. It is impossible to believe that the time given can be much too early, and therefore

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the corresponding distance of centres 15′ 41″·08 cannot be much too small.

I will exhibit the values of the solar parallax which would be required to satisfy the equations. (1) Taking the first set of observations at Rodriguez, we have the following equations:—

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{3}\,(x+0{\cdot}0270\,y) = \sqrt{3}\times 0{\cdot}327,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080.$$

These lead to the following equations for the determination of x and y:—

$$x+0{\cdot}1052\,y = 0{\cdot}3825,$$
$$x+0{\cdot}2336\,y = 0{\cdot}5461;$$

whence

$$x = 0''{\cdot}249,\ y = +1{\cdot}274,\ \text{and parallax} = 9''{\cdot}06.$$

(2) Taking the second set at Rodriguez, we have

$$\sqrt{4}\,(x+0{\cdot}2020\,y) = \sqrt{4}\times 0{\cdot}455,$$
$$\sqrt{3}\,(x+0{\cdot}1910\,y) = \sqrt{3}\times 0{\cdot}543,$$
$$\sqrt{2}\,(x+0{\cdot}0270\,y) = \sqrt{2}\times 0{\cdot}575,$$
$$\sqrt{2}\,(x-0{\cdot}1000\,y) = \sqrt{2}\times 0{\cdot}080,$$

which give for the determination of x and y the equations

$$x+0{\cdot}1123\,y = 0{\cdot}4326,$$
$$x+0{\cdot}2381\,y = 0{\cdot}5618;$$

whence

$$x = +0''{\cdot}317,\ y = +1{\cdot}027,\ \text{and parallax} = 9''{\cdot}04.$$

One of the results—9″·04 or 9″·06, or their mean—would, in my opinion, have to be accepted as the best value deducible from these Egress observations, were it not that we are compelled to recognise that the observations at Kerguelen were caught up with great difficulty through clouds, and that we have a later observation of Perry's, which, although called a contact, is completely separated off from the apparent contacts at the other stations, and must therefore be accepted as an observation much nearer the time of so-called real contact; and we have already pointed out that the observations in Egypt have been made when the ligament or black drop has been very largely developed. I will next take Perry's observation of contact as that of ligament well marked, and use with it the second set at Rodriguez. This is a most extreme supposition, and would not be worth consideration were it not for the fact that the observations at Thebes, which are clearly not observations of apparent contact in the same sense as

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those made at the other stations, are yet called "contacts," and are evidently the same phase as that observed at Mokattam and Suez, where it is clear that the times have been pushed to an extreme limit by the observers waiting for the ligament to assume a sufficient degree of blackness. These considerations do perhaps to some extent justify the use of Perry's observation to the exclusion of those of Sidgreaves and Goodridge, but such an exclusion is certainly not satisfactory. On this supposition we have

$$\sqrt{4}\,(x + 0{\cdot}2020\,y) = \sqrt{4} \times 0{\cdot}455,$$
$$\sqrt{3}\,(x + 0{\cdot}1910\,y) = \sqrt{3} \times 0{\cdot}543,$$
$$\sqrt{2}\,(x + 0{\cdot}0270\,y) = \sqrt{2} \times 0{\cdot}575,$$
$$\sqrt{1}\,(x - 0{\cdot}1000\,y) = \sqrt{1} \times 0{\cdot}680,$$

which lead to the equations

$$x + 0{\cdot}1335\,y = 0{\cdot}5279,$$
$$x + 0{\cdot}2128\,y = 0{\cdot}4807;$$

whence

$$x = +0''{\cdot}607,\ y = -0{\cdot}591,\ \text{and parallax} = 8''{\cdot}897.$$

This value, like the value $9''{\cdot}05$, must be regarded as resting upon an extreme assumption, but in an opposite direction to the former one. But, on account of the existence of clouds at Kerguelen and the impossibility of discriminating between the kinds of contact observed, we should perhaps combine at all the stations attempts to observe real contacts without any discrimination between the finer phenomena presented. But if we look at Goodridge's observation made at $11^h\ 1^m\ 39^s{\cdot}9$, we shall see that, although the limbs were only just visible through cloud, some decided connection was already established. I am hardly prepared, therefore, to consider that this observation can be fairly classed with such observations of a very early phase as that of Mrs. Campbell, which was made under the most favourable atmospheric circumstances. But as I wish to exhibit clearly the extreme limits within which the values of the solar parallax must be confined when deduced from any legitimate assumption, I will discuss these contact observations, both excluding and including the very early ones. It will be seen that the two solutions agree very closely.

The result of a discussion of the four very early observations by themselves in a separate class is not worth consideration when the circumstances under which Goodridge's observation was made are considered.

The following solution is based upon the mean of all attempts to observe real contacts, excluding only attempts at apparent contacts, which are separated at the stations at which

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both are observed by more than a minute of time from the phase used, and whose results could not therefore, under any circumstances, be admitted in the equations of condition. The following are the equations on this assumption:—

$x + 0{\cdot}2020\,y =$	$0{\cdot}18$	5 obs.
$x + 0{\cdot}1910\,y =$	$-0{\cdot}04$	5 obs.
$x + 0{\cdot}0270\,y =$	$0{\cdot}24$	6 obs.
$x - 0{\cdot}1000\,y =$	$0{\cdot}12$	4 obs.

The number of observations at the different stations are so nearly equal that we shall assume the weights to be equal, and we have

$$x + 0{\cdot}0800\,y = 0{\cdot}1250,$$
$$x + 0{\cdot}2750\,y = 0{\cdot}0725;$$

whence

$$x = 0''{\cdot}147,\ y = -0{\cdot}27,\ \text{and parallax} = 8''{\cdot}93.$$

The residual errors are

Obs.—Comp.	
$+0{\cdot}087$	Mokattam and Suez,
$-0{\cdot}135$	Thebes,
$+0{\cdot}100$	Rodriguez,
$-0{\cdot}054$	Kerguelen.

There remains only a solution obtained by combining all the observations from "ligament clearly seen" to so-called "contacts," but excluding the very early observations of shadows or interference lines, a phase which is only caught by a single observer at each station, and which should therefore be excluded from our equations of condition from this consideration alone. On this supposition our equations of condition are

$$\sqrt{4}\,(x + 0{\cdot}2020\,y) = \sqrt{4} \times 0{\cdot}455,$$
$$\sqrt{4}\,(x + 0{\cdot}1910\,y) = \sqrt{4} \times 0{\cdot}368,$$
$$\sqrt{5}\,(x + 0{\cdot}0270\,y) = \sqrt{5} \times 0{\cdot}426,$$
$$\sqrt{3}\,(x - 0{\cdot}1000\,y) = \sqrt{3} \times 0{\cdot}283.$$

From these we get

$$x + 0{\cdot}0879\,y = 0{\cdot}3919,$$
$$x + 0{\cdot}2436\,y = 0{\cdot}4416;$$

whence

$$x = +0''{\cdot}364,\ y = 0{\cdot}319,\ \text{and parallax} = 8''{\cdot}979.$$

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The residual errors are

Obs.—Comp.	
"	
+0·027	Mokattam and Suez,
−0·057	Thebes,
+0·053	Rodrigues,
−0·049	Kerguelen.

These residual errors are very small, and this appears to be the only satisfactory way of obtaining any good *independent* value from these Egress observations. The value thus found is larger than we have reason to believe it should be, but the probable error of the determination is considerable. From these Egress observations, then we have,

$$\pi = 8''{\cdot}979 \pm 0{\cdot}279 \times e,$$

where e has a similar meaning to that in the Ingress observations.

It appears to me that these Egress observations cannot be satisfied in any way by a value less than 8″·88, and that the value 8″·98 is that which is derived from them by the most satisfactory *à priori* assumption of corresponding observations. Accepting, therefore, for the Ingress $8''{\cdot}860 \pm 0{\cdot}136\ e$, and for the Egress $8''{\cdot}979 \pm 0{\cdot}279\ e$, we have for the mean solar parallax

$$(5) \qquad 8''{\cdot}884 \pm 0{\cdot}123 \times e$$

as the value deducible from these contact observations, a value which I believe absolutely negatives any value smaller than 8″·84 or larger than 8″·93.

My own earlier determinations are

	"	
(1) From observations of *Mars* in 1862	8·94	Published 1863.
(2) From parallactic inequality in Earth's motion. A value of small weight from its depending on the assumed value of the Moon's mass.	8·91	„ 1867.
(3) From parallactic inequality in Moon's motion. Deduced from 2075 observations near the times of maximum effect.	8·85	„ 1867.
(4) From a rediscussion of Transit of *Venus* Observations, 1769	8·91	„ 1868.

These five determinations give me

$$\pi = 8''{\cdot}897 \pm 0''{\cdot}020.$$

Besides these I give for comparison with my results the following:—

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Dr. Winnecke, from the *Mars* observations in 1862, deduced a value 8″·96. This was published in 1863.

Mr. Newcomb's discussion of the *Mars* observations in 1862, published in 1867, gave 8″·855. The smaller value deduced from these *Mars* observations by Mr. Newcomb depends chiefly on the use of the Santiago observations, where the inclination of the wire used was not determined. Mr. Newcomb has attempted to eliminate the errors thus introduced, but I think that the use of these Santiago observations is, under the circumstances, inadmissible.

Hansen's value, deduced from the parallactic inequality in the Moon's motion, is 8″·916. This value was first published in 1867. Hansen had intimated several years before that the parallactic inequality required an increase, but it is doubtful whether he was prepared at that time to throw the whole of this required increase upon an error in the assumed value of the solar parallax. There are also the determinations from the velocity of light which have lately been repeated by M. Cornu, and which, with the accepted value of the coefficient of aberration, lead to a value 8″·86. And there are the classical investigations of Le Verrier, in which, starting with an erroneous value of the ratio of the Sun's mass to that of the Earth deduced from Encke's value of the solar parallax, he not only showed from the theories of the planets themselves that the assumed value of the solar parallax was incorrect, but deduced a value 8″·88, which is entitled to great weight, and is in most satisfactory agreement with the values deduced more directly from observation. If we assume $\pi = 8''{\cdot}89$, the correction which this assumed value requires must be very small. This will correspond to a distance of the Earth from the Sun equal to 91,940,000 miles.

Royal Observatory, Cape of Good Hope,
1878, *January* 18.

On Certain Groups of Stars with Common Proper Motions.

By Prof. T. H. Safford.

It has been long noticed by those who have given attention to the subject of proper motion that there are many groups of stars which seem to have a common proper motion. This tendency has been called star-drift by Mr. Proctor; and the special instance which he has brought prominently forward is that of the group of five stars in *Ursa Major*, β, γ, δ, ϵ, ζ. The motion of these stars is nearly in the same direction, nearly opposed to that which results from the solar motion, but varies in amount from about 0″·09 to 0″·15 yearly.

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Upon this subject future determinations will doubtless throw a great deal of light. I am inclined to think that the observations and calculations of the next few years will do much in this direction, as we can now combine good modern observations with ancient ones, not only of Bradley, Lalande, Piazzi, and Groombridge, but also of Struve, Pond, Bessel, and Argelander.

Of the 68 stars within 10° of the North Pole which are reckoned visible to the naked eye (either by Argelander, Heis, or Carrington) about one-half have proper motions whose determination can now be effected with some accuracy. I have already deduced them in a provisional manner, in order to be able to compute more exactly the secular variations and terms of higher orders of the whole motion; or, in other words, to be able to establish formulæ which shall nearly represent observation and be easily corrected.

There are several groups which have proper motions nearly alike in amount and direction.

The first group to which I shall call attention is composed of six stars, whose names (according to Argelander), B.A.C. Nos., rough positions for 1855, and annual proper motions here follow:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	$\Delta\alpha$. s	$\Delta\alpha \cos\delta$. ″	$\Delta\delta$. ″
36 *Cephei* H.	8026	22 55·4	83 33·2	+0·05	+0·08	+0·02
39 *Cephei* H.	8213	23 27·8	86 30·4	+0·085	+0·08	+0·01
Bradley 3187	8321	23 49·7	82 23·0	+0·03	+0·06	0·00
Bradley 74	225	0 41·6	82 55·1	+0·04	+0·07	−0·01
43 *Cephei* H. (2 *Ursæ Minoris*).	262	0 49·7	85 28·6	+0·068	+0·08	0·00
Bradley 95	273	0 52·8	86 22·2	+0·07	+0·06	−0·01

It will be seen that a mean proper motion of +0″·07 and 0″·00 is almost as exact for the individual stars as the proper motions can be depended upon, and that the slight deviations in declination indicate that the common motion of all these stars is nearly perpendicular to the equinoctial colure.

Polaris itself has a less proper motion in nearly the same direction; and the small star B.A.C. 240 which precedes it shows traces of a near relationship in its motion, as follows:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	$\Delta\alpha$. s	$\Delta\alpha \cos\delta$. ″	$\Delta\delta$. ″
Bradley 65	240	0 45·4	88 14·6	+0·11	+0·05	−0·02
Polaris	360	1 6·5	88 32·2	+0·11	+0·04	0·00

It seems, however, unlikely that the same value of $\Delta\delta$ will represent as fully as is desirable the observations of both stars. This point must be decided by a fuller study.

In the third group are contained three stars:—

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Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
Bradley 344	784	2 27·2	80 49·6	+0·02	+0·05	−0·08
Bradley 402	960	3 58·8	84 23·1	+0·04	+0·06	−0·09
Radcliffe 1311	...	4 41·0	85 45·2	+0·04	+0·04	−0·09

The parallelism of motion here is not quite so striking, but will need further observations either to prove or disprove.

Two other stars make a pretty sure pair, namely:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
Groombridge 1850	4070	11 57·4	86 23·4	−0·06	−0·05	+0·06
6 *Ursæ Minoris* B.	4165	12 14·4	88 30·2	−0·094	−0·04	+0·07

Still two others are quite doubtful, although agreeing in amount more nearly than the two stars β and ζ *Ursæ Majoris*:—

Name.	No. B.A.C.	A.R. h m	Decl. ° ′	Δα. s	Δα cos δ. ″	Δδ. ″
30 *Camelop.* H.	3528	10 13·0	83 17·6	−0·052	−0·09	+0·03
202 *Camelop.* B.	3906	11 21·4	81 58·5	−0·06	−0·13	+0·04

As to the dependence to be placed on these numbers, it is naturally not very large. They are, however, pretty fairly substantiated by the evidence, which in nearly all the cases is abundant. How they were discussed may be seen from the following table for the last star mentioned, giving the positions reduced to 1855:—

	h m s	° ′ ″	O−C. s	O−C. ″
Groombridge	11 21 28·9	81 58 28·1	−0·1	−0·1
Struve 1815	28·3	...	+0·2	...
Schwerd 1828	27·5	29·1	+0·2	−0·4
Radcliffe	26·8	28·3	−0·1	+1·1
Yarnall 1846·8	27·1	...	−0·5 (1 observation).	...
Carrington	26·4	30·1	−0·3	−0·3
H.C. 1865	25·5	...	0·0	...
Yarnall 1869	...	30·6	...	−0·2

These figures were obtained at second-hand in several cases, so I only wished to gain approximate positions and proper motions in order to calculate precessions and secular variations (the latter including terms depending on proper motion) with the needful precision. The larger proper motions in this region are mostly well known. The star 223 *Camelop.* Bode = B.A.C. 4982 is the chief exception to this statement. Its annual proper motion is about +0ˢ·10 in A.R. and −0″·27 in Declination, which, at 6°·9 from the Pole, amounts to rather more than 0″·3 in arc.

Williams College, Williamstown, Mass., U.S.A.
1878, *January* 12.

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Ephemeris for Physical Observations of Jupiter, 1878. By A. Marth, Esq.

0h G.M.T.	Angle of Position of ♃'s Axis.	Longitude of ♃'s Meridian directed to the Earth.	Diff.	Latitude of Earth above ♃'s Sun Equator.		Annual Parallax.	Equat. Diam.	Greatest Phase. Prec. side.	Corr. of Long.
1878.	°	°	°	°	°	°	"	"	°
Apr. 25	343·89	126·04		−0·74	−1·08	−11·28	38·71	0·374	+0·55
			4352·98						
30	343·74	159·02		·71	1·06	11·28	39·33	·380	·55
			3·05						
May 5	343·62	192·07		−0·68	−1·04	−11·20	39·96	·381	+0·55
			3·12						
10	343·52	225·19		·65	1·02	11·05	40·61	·376	·53
			3·19						
15	343·45	258·38		·63	1·00	10·83	41·27	·368	·51
			3·25						
20	343·41	291·63		·61	0·98	10·53	41·93	·353	·48
			3·32						
25	343·39	324·95		·59	·95	10·14	42·60	·333	·45
			3·39						
30	343·40	358·34		·58	·93	9·67	43·25	·307	·41
			3·45						
June 4	343·44	31·79		−0·56	−0·91	−9·13	43·89	·278	+0·36
			3·50						
9	343·51	65·29		·55	·89	8·51	44·51	·245	·32
			3·56						
14	343·60	98·85		·54	·87	7·81	45·10	·209	·27
			3·60						
19	343·71	132·45		·54	·85	7·04	45·65	·172	·22
			3·64						
24	343·85	166·09		·53	·82	6·21	46·15	·135	·17
			3·68						
29	344·02	199·77		·53	·80	5·31	46·60	·100	·12
			3·69						
July 4	344·20	233·46		−0·53	−0·78	−4·36	46·99	·068	+0·08
			3·71						
9	344·40	267·17		·54	·76	3·36	47·30	·041	·05
			3·71						
14	344·62	300·88		·54	·74	2·33	47·54	·020	·02
			3·71						
19	344·85	334·59		·55	·71	1·27	47·70	·006	+ ·01
			3·68						
24	345·08	8·27		·56	·69	−0·19	47·77	foll. side	·00
			3·64						
29	345·32	41·91		·57	·67	+0·88	47·76	·003	·00
			3·58						
Aug. 3	345·56	75·49		−0·58	−0·65	+1·95	47·66	·014	−0·02
			3·54						
8	345·79	109·03		·59	·62	3·00	47·47	·033	·04
			3·47						
13	346·02	142·50		·60	·60	4·02	47·21	·058	·07
			3·39						

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♂ G.M.T.	Angle of Position of ♃'s Axis.	Longitude of ♃'s Meridian directed to the Earth.	Diff.	Latitude of Earth above ♃'s Sun Equator.		Annual Parallax.	Equat. Diam.	Greatest Phase. Prec. side.	Corr. of Long.
1878.	°	°	°	°	°	°	″	″	°
Aug. 18	346·23	175·89		·61	·58	4·99	46·87	·089	·11
			3·31						
23	346·42	209·20		·62	·55	5·91	46·47	·123	·15
			3·22						
28	346·59	242·42		·63	·53	6·77	46·00	·161	·20
			3·13						
Sept. 2	346·73	275·55		−0·64	−0·51	+7·57	45·49	·198	−0·25
			3·02						
7	346·84	308·57		·64	·49	8·30	44·93	·235	·30
			2·93						
12	346·93	341·50		·65	·46	8·95	44·33	·270	·35
			2·82						
17	346·98	14·32		·65	·44	9·52	43·71	·301	·40
			2·73						
22	347·01	47·05		·65	·42	10·02	43·08	·329	·44
			2·63						
27	347·00	79·68		·65	·39	10·43	42·43	·351	·47
			2·53						
Oct. 2	346·96	112·21		−0·65	−0·37	+10·75	41·77	·367	−0·50
			2·44						
7	346·88	144·65		·65	·35	11·00	41·12	·378	·53
			2·36						
12	346·78	177·01		·64	·33	11·17	40·48	·383	·54
			2·27						
17	346·64	209·28		·64	·30	11·26	39·85	·383	·55
			2·20						
22	346·48	241·48		·63	·28	11·28	39·23	·379	·55
			2·13						
27	346·29	273·61		·61	·26	11·23	38·63	·370	·55
			2·06						
Nov. 1	346·08	305·67		−0·60	−0·23	+11·11	38·05	·365	−0·54
			2·00						
6	345·84	337·67		·58	·21	10·93	37·50	·340	·52
			1·94						
11	345·59	9·61		·56	·19	10·68	36·97	·320	·50
			1·90						
16	345·31	41·51		·54	·16	10·38	36·47	·298	·47
			4351·86						
21	345·02	73·37		−0·52	−0.14	+10·03	36·00	0·275	−0·44

Assumed daily rate of rotation 870°·60. The "annual parallax" is the difference of the Jovicentric longitudes of the Sun and Earth, reckoned in the plane of *Jupiter's* equator. The last column gives the correction, which is to be applied to the "longitude of the meridian directed to the Earth," in order that it may refer to the meridian which bisects the illuminated disk. The inclinations γ and the ascending nodes Γ of the orbits of the satellites in reference to the plane of *Jupiter's* equator are the following, the nodes being reckoned from the descending node of the equator on the planet's orbit, or from the vernal equinox of *Jupiter's* northern hemisphere:—

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1878.	Sat. I. γ_1	Sat. I. Γ_1	Sat. II. γ_2	Sat. II. Γ_2	Sat. III. γ_3	Sat. III. Γ_3	Sat. IV. γ_4	Sat. IV. Γ_4
	°	°	°	°	°	°	°	°
Feb. 24	0·0097	30·8	0·4553	37·42	0·1942	271·14	0·3254	330·22
April 25	97	28·7	·4559	35·40	·1936	270·60	·3250	330·12
June 24	96	26·6	·4566	33·37	·1930	270·08	·3247	330·01
Aug. 23	96	24·4	·4573	31·35	·1924	269·58	·3245	329·90
Oct. 22	96	22·1	·4581	29·32	·1918	269·10	·3244	329·80
Dec. 21	0·0096	19·7	0·4589	27·29	0·1913	268·64	0·3245	329·70

Correction to the Ephemeris of the Satellites of Uranus.

On page 97 the values of the position angles p_e are, by mistake, wrongly given. The correct values are as much above 10° as the printed ones are below 10°, or the true values are found by subtracting the printed ones from 20°. Thus

	°		°
For	8·13	*read*	11·87
„	8·19	„	11·81
	*	*	*
„	8·95	„	11·05.

Observations of Occultations of Stars by the Moon, and of Phenomena of Jupiter's Satellites, made at the Royal Observatory, Greenwich, in the year 1877.

(*Communicated by the Astronomer Royal.*)

Occultations of Stars by the Moon.

Day of Obs. 1877.	Phenomenon.	Telescope.	Power.	Moon's Limb.	Mean Solar Time of Observation. h m s	Observer.
Jan. 30	Reapp. of 45 Leonis	Altaz.	100	Dark	9 15 50·3	A.D.
„	Disapp. of ρ Leonis	„	„	Bright	10 51 19·4	„
„	Reapp. of ρ Leonis	„	„	Dark	11 57 2·8	„
Feb. 26	Disapp. of Regulus	E. Eq.	140	„	12 45 43·8	M.
„ (*a*)	„ „	S.E. Eq.	285	„	12 45 44·0	W.C.
„	„ „	Altaz.	100	„	12 45 44·3	A.D.
Mar. 21	„ Piazzi V: 136	„	„	„	10 20 31·9	T.
Nov. 20 (*b*)	„ 19 Tauri	S.E. Eq.	130	Bright	7 42 19·7	M.
„ (*c*)	„ 20 Tauri	„	„	„	7 49 40·0	„
„	Reapp. of 17 Tauri	„	„	„	8 10 31·1	„
„	„ 19 Tauri	„	60	„	8 39 10·9	„
„ (*d*)	„ 20 Tauri	„	„	„	8 55 55·5	„

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Phenomena of Jupiter's Satellites.

Day of Obs. 1877.	Satellite.	Phenomenon.	Telescope.	Power.	Mean Solar Time of Observation. h m s	Mean Solar Time from *N.A.* h m s	Observer.
May 7 (*e*)	II	Occ. reapp. bisection	S.E. Eq.	220	13 4 25·1		M.
„	II	„ last cont.	„	„	13 6 27·8		„
„ (*f*)	II	„ first cont.	E. Eq.	70	13 1 0·4	13 0	L.
„	II	„ bisection	„	„	13 2 36·5		„
„	II	„ last cont.	„	„	13 5 32·4		„
June 20 (*g*)	I	Tr. ing. last contact	„	140	10 48 36·7	10 50	T.
July 20	I	„ first contact	„	„	12 13 24·7	12 16	A.D.
„	I	„ last contact	„	„	12 16 34·2		„
24 (*h*)	II	Occ. disapp. last cont.	„	„	11 12 11·3	11 11	„
26 (*i*)	II	Tr. egr. bisection	„	„	9 2 38·0	9 3	B.D.
„	II	„ last contact	„	„	9 5 47·5		„
29 (*k*)	I	„ first contact	„	„	10 42 38·0		C.
„	I	„ bisection	„	„	10 43 37·8	10 45	„
„	I	„ last contact	„	„	10 45 7·6		„
30	I	Ecl. reapp. first seen	S.E. Eq.	285	8 56 32·1		M.
„	I	„ full brightness	„	„	8 56 57·1	8 56 37	„
„	I	„ full brightness	E. Eq.	140	8 57 0·9		R.
Sept. 5	II	Ecl. reappearance	„	„	7 14 1·2	7 14 36	J.B.

Notes.

(*a*) The star disappeared instantaneously; sky cloudy.
(*b*) The Moon's limb was exceedingly tremulous. The star was lost to view several times before reaching the limb, and finally disappeared in a hollow.
(*c*) The time of disappearance was very difficult to ascertain, owing to the glare of the Moon, which was sensibly full.
(*d*) The Moon's limb was fairly steady.
(*e*) *Jupiter* was very tremulous, and the limb badly defined.
(*f*) Image bad; the observation very unsatisfactory.
(*g*) The definition of the planet was bad, owing to light cloud.
(*h*) The observation unsatisfactory; a diffused light round *Jupiter*.
(*i*) *Jupiter* very tremulous, and the observation uncertain.
(*k*) Image bad.

The clear aperture of the object-glass of the S.E. Equatoreal is 12¾ inches, of the East Equatoreal 6·7 inches, and of the Altazimuth 3¾ inches.

The initials W.C., L., C., A.D., M., T., B.D., R., and J.B. are those of Mr. Christie, Mr. Lynn, Mr. Criswick, Mr. Downing, Mr. Maunder, Mr. Thackeray, Mr. Dennison, Mr. Robinson, and Mr. Bromley.

Royal Observatory, Greenwich,
1878, *February* 13.

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On Mr. Stone's Note in the January Number of the Monthly Notices. By Richard A. Proctor, B.A., Hon. Fell. King's College, London.

Mr. Stone calls my attention to a Paper by Sir G. Airy, in vol. xxiv. of the *Monthly Notices*, showing that the part of the disk of the Sun seen through a refracting medium, causing an effect analogous to that of a convex lens, will not be of greater apparent brightness than the rest. He is right in presuming that I was unacquainted with Sir G. Airy's investigation when my paper was written; but not quite so in saying that the investigation covers the ground of my paper, if at least he correctly sums up its results as this, that "no refraction can cause a change in the apparent brightness of the surface visible." For in my paper I considered the possibility of reflexion as well as refraction, and of various conceivable combinations of both reflexion and refraction. In the article entitled "Notes on Brightness," referred to in my paper, I considered the effect of all possible combinations of reflexion and refraction, including, though not specially mentioning, the case of refraction through prisms, which some seem to suppose competent to increase the absolute apparent lustre of a surface. This cannot actually happen, though the relative lustre of an object and its surroundings may be so modified that the visibility of the surface may be greatly strengthened, or (as in the case of the solar prominences) an object rendered visible which was before undiscernible.

My remarks related entirely to intrinsic surface brightness. So that, as far as nebulæ are concerned, they only apply to cases where (whether by varieties of telescopic power or of distance) a nebula is neither so magnified that individual component stars are seen, nor so reduced in size as to seem a mere point. In the former case the individual stars necessarily appear brighter with increase of telescopic power, or diminution of distance; in the latter the nebula as a whole does so. It is hardly necessary to say that the satellites of *Mars*, appearing as mere points in the most powerful instruments yet made, cannot be used to illustrate the subject of apparent intrinsic brightness. I was so careful to draw a distinction between intrinsic brightness and quantity of light, that I cannot recognise the justice of the distinction drawn by Mr. Stone between what I said and what I probably meant. He asks why, if my statement (ending with the words that "quantity of light can of course be greatly increased") were true, the satellites of *Mars* could not as easily be seen with a 4-inch as with a 26-inch telescope; a question which actually contains its own answer, viz. that quantity of light is of course greatly increased when the larger telescope is used.*

* At the meeting when the present paper was read, it was suggested that Mr. Stone may have referred to the different dimensions of the diffraction disks

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Unless, indeed, it be necessary to explain that the visibility of an object depends in some degree on the quantity of light we receive from it.

I am sorry, on the other hand, to have misinterpreted Mr. Stone's own remarks about gaseous nebulæ. Without, however, for a moment questioning his having really intended his paper to be understood as he explains (in fact, few actions can be more discourteous or unjust than to question anyone's explanation of his own words), I must say that it does not seem to me that he succeeded in clearly indicating his meaning in his former paper. He not only did not dwell upon, but he did not even mention the physical processes which would result from the actual, as distinguished from the optical, overlapping of the gaseous envelopes surrounding the component stars of the clusters he considered. It was a natural mistake, therefore, to suppose that his remarks referred only to optical over-lapping. My remarks applied only to his reasoning so misunderstood.

Observations of Shooting Stars in 1877. By W. F. Denning, Esq.

The following table of radiant points observed in 1877 is presented as a continuation of my paper in the *Monthly Notices*, vol. xxxvii., pp. 105–115, giving a similar list of results for 1876. In 1877 the paths of 1,929 shooting stars were registered, and of this total the great majority (1,593) were seen during the last half of the year. The estimated magnitudes were as follow:—

> 1st.	= 1st.	= 2nd.	= 3rd.	= 4th.	= 5th.		Total.
45	118	346	491	636	293	=	1,929

Observations were made nearly every fine night in the absence of moonlight, and were about equally divided in the aggregate between the morning and evening hours. In the table of radiants I have given the number of meteors traced from each centre, and have affixed notes to them, which render any detailed preliminary references unnecessary. The chief radiants were:—

No.	Period.	Radiant. ° °	No. of ↓s.	No.	Period.	Radiant. ° °	No. of ↓s.
1	Jan. 4-20	295+53	20	97	Oct. 2—Nov. 13	104+49	36
49	July 6—Aug. 16	7+37	37	99	Oct. 2—Nov. 13	105+12	33
62	July 6—Aug. 16	334+44	24	106	Oct. 2—Nov. 13	113+27	21
53	July 6—Aug. 16	7+53	20	117	Oct. 2—Nov. 13	101+73	23
52	July 6—Aug. 16	315+50	21	113	Oct. 2-18	133+79	22
65	Aug. 10	43+58	285	89	Oct. 15-20	92+1	57
109	Sept. 4—Oct. 19	84+54	26	118	Oct. 15—Nov. 13	130+1	30
108	Sept. 4—Oct. 19	108+38	20	122	Oct. 28—Nov. 13	43+2	31

of the satellites as seen with large or small telescopes. But I find nothing in Mr. Stone's own words which can be so interpreted.

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In getting the radiants I have been careful to consider the lengths of the apparent paths and specialities of motion and appearance in the various meteors. When this is carefully done there is little danger of *pseudo*-radiants, even in cases of feeble showers. A proportion of new radiants will be found amongst my observations.

Generally I find meteors far more abundant during the last half of the year than during the first half, but my notes chiefly have reference to the former period, so that I cannot give a fair comparison. For the six months, July–December, the following are the periods of watching and number of meteors seen (omitting the *Perseïds*) during the last two years:—

Month 1896–77.	Obs. A.M. Hours.	Meteors.	Obs. P.M. Hours.	Meteors.	Obs. A.M. & P.M. Hours.	Meteors.
July	13	133	24¼	166	37¼	299
August	17¼	271	30	303	47¼	574
September	12	146	15¼	118	27¼	264
October	24¾	366	47	362	71¾	728
November	24¼	290	34¼	278	58½	568
December	11	139	24¾	157	35¾	296
July—Dec.	= 102¼	1345	175½	1384	277¾	2729

The table shows the remarkable frequency of shooting stars in the morning as compared with the evening apparitions. In any calculation of the horary numbers from these figures, one-sixth of the periods of watching should be deducted as allowance for time spent in registering the paths.

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No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
1	January 4–20	295	+53	20	Strong shower; maximum Jan. 17; meteors bright, rather slow.
2	January 4–20	57	−12	7	Very fine, slow meteors; maximum Jan 4–8; radiant near γ *Eridani*.
3	January 4–20	220	+52	5	Meteors rather slow; also a radiant at 231° + 54°, Jan. 4 (*Quadrantids*).
4	January 4–20	209	+67	7	Rapid white meteors; near α *Draconis*.
5	January 14–17	260	+45	6	Short, small meteors; a morning shower.
6	January 4–20	248	+73	7	Very slow meteors; new shower near γ *Ursæ Minoris*.
7	January 4–20	84	+74	6	Uncertain. Short, quick meteors. 110° + 70°, Jan. 27, 1868 (Zezioli's obs).
8	January 4–20	130	+44	11	Strong shower; maximum Jan. 15 A.M. Bright meteors.
9	January 4–20	250	+25	10	A feeble morning shower in *Hercules*. Comet of 1672, 256° + 20°, Jan. 20.
10	January 4–20	209	+15	4	Probably two separate showers A.M.; near α *Boötis*. Slightly seen in Jan. 1878 also.
	January 14–20	211	+ 4	5	Swift meteors, with streaks. Tupman, 200° + 2°, Jan. 8–11, 1870.
11	January 14–20	179	+47	9	Uncertain. Confirms S. & Z. 175° + 48°, Jan. 6.
12	January 4–20	160	+28	6	Very swift, white meteors.
13	February 15–20	236	+11	10	Exact. Strong shower A.M., near α *Serpentis*.
14	February 20	263	+36	6	Exact. A pair of rich A.M. showers; rapid bright meteors.
15	February 20	181	+34	8	Exact. Corder, 263° + 36°, January 27, 1877.
16	February 15–20	261	+ 4	3	Feeble; uncertain. Meteors with streaks; radiant near β *Ophiuchi*.
17	February 15–21	210	−13	5	Uncertain. Meteors very swift. Tupman, 202° − 9°, February 13.
18	February 4–20	167	+ 5	5	Meteors slow. Tupman, 165° + 4°, January 8–10.
19	February 2–4	105	+29	4	Meteors slow; rather uncertain. Near α *Geminorum*.

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No. 1877.	Date. 1877.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
		°	°		
20	February	234	+ 32	12	Well defined and active shower. Confirmed by other observations.
21	February	120	− 5	9	A new shower in *Monoceros*.
22	March 14	263	+ 48	6	Exact. Confirms S. & Z. 260° + 46°, March 31.
23	March 14	218	+ 36	6	Active shower; meteors slow; near *β Boötis*.
24	March 14–19	320	+ 44	4	Morning showers; near *α Cygni*.
	April 16–19	315	+ 38	10	Confirms Greg (49), 307° + 40°, March 15—April 21.
25	May 15–17	299	+ 36	4	Confirms S. & Z. 301° + 37°, May 22–24. ? same as preceding.
26	March 8	189	+ 26	6	Active shower. Confirms S. & Z. 187° + 24°, April 11–19.
27	March	175	+ 10	5	A very good shower of bright meteors.
28	March 17	252	+ 12	3	Exact. One meteor nearly stationary.
29	March 17	330	+ 33	?	Strongly suspected. Corder, 325° + 34°, March.
30	April 16–19	303	+ 13	8	= Comet V. 1857, 302° + 11°, April 4. Confirmed by other observations.
31	May 15	304	+ 7	4	Slightly seen. Distinct to the preceding?
32	March 14	280	− 14	2	Meteors very swift; one long, continued shower.
	April 16–19	289	− 15	4	Confirms Greg (52), 290° − 10°, March 25—April 30.
33	March 1–15	300	+ 80	9	Bright, slow meteors; exact.
34	April 16–19	286	+ 5	12	Rather uncertain. = Comet II. 1844, 288°·5 + 5°, April 21.
35	March 14	277	+ 25	5	Exact. Well seen, A.M.; near *β Lyræ*.
36	April 16–19	267	+ 21	6	Confirms Greg, April 20, 267° + 25°.
37	April 16–19	298	+ 25	6	A new A.M. shower in *Vulpecula et Anser*.

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38	April 16–19	260 + 2	4	Confirms Tupman, 256° − 2°, April 27.
39	April 16–19	228 − 2	5	Exact. A rich shower. Confirms G. & H. (53), 227° − 5°, Mar. 20–May 29.
40	April 16–19	269 + 37	7	*Lyraids.* At 274° + 37° in 1873 and 1874. = Comet I. 1861.
41	March 14	244 + 50	6	These showers are possibly connected together. Mean position at 250° + 51°. S. & Z. 246° + 46°, April.
	March 14	250 + 54	4	
	April 16–19	255 + 50	5	
42	April 16–19	218 + 25	4	Uncertain. Probably the same shower. Seen at 220° + 21° on April 21, 1874. (Greg 53*a*.)
	May 7	219 + 17	3	
43	May 15–17	354 + 41	4	Heis, 355° + 42°, May 20–29. An A.M. shower.
44	May 17	283 + 55	4	*Draconids.* Short, rapid meteors. Seen also in 1873 at 283° + 59°, April 21.
45	April—May	258 − 12	11	*Serpentids.* Active shower. Greg (80–85), 264° − 11°, June, July, and Aug.
46	May	210 − 10	3	A rich and probably new shower.
	April—May	203 − 8	?	Seen also by Corder at 208° − 6°, April—May 1877.
47	June 15–17	302 + 64	5	Not very certain.
48	June 15–17	285 + 32	4	Confirms Tupman, 280° + 29°, June 29, 30.
49	July 6–17	4 + 35	21	A very active shower; radiant not very precise. *Andromedes* I.
	August 3–16	10 + 38	16	Maximum about July 7–11. Greg (103), 7° + 33°, July 21—September 3. = Comet II. 1870, 3°·5 + 38°·5, August 14–.
50	July 6–17	0 + 26	4	Distinct to preceding. Radiant near *α Andromedæ.*
	August 3–16	4 + 20	7	Denza, 2° + 29°, August 8–13.
51	July 6–17	271 + 8	5	Uncertain. Meteors very slow.
52	July 6–17	315 + 48	8	Active shower; radiant near *α Cygni. Cygnids* I.
	August 3–16	315 + 51	13	Swift, white meteors. Greg (81), 309° + 48°, July—August.

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No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
53	July 6–17	6	+53	11	Exact. A fine shower of *Cassiopeids*; maximum about July 12–17. Streaks.
	August 3–16	8	+53	9	Swift, white meteors with streaks. S. & Z., 7°+50°, July 11.—Aug. 7.
54	July 6—August 16	350	+37	11	Active shower; possibly new.
55	July 6–17	290	+43	8	Well-marked radiant near δ *Cygni*.
	August 3 16	292	+48	7	Confirms G. and H., 293°+42°, August 10, 1871.
56	July 6—August 16	349	+53	14	Observed chiefly in July. Short, quick meteors.
57	July 6–20	317	−11	13	Strong maximum on July 20. Tupman, 313°−6°, July 21, 22.
58	July 6–17	336	− 7	5	The *Aquarids*. Greg (109), 337°−6°, July 5—October 31!
59	August 3–16	342	−12	10	A distinct shower to the preceding; near α *Aquarii*.
60	July 6—August 16	333	+26	11	Chiefly observed in July. Greg (96), 338°+26°, July—August 15.
61	July 6–17	338	+10	10	Confirms Greg (97), 338°+17°, June 28—September 15.
	August 3–16	343	+12	8	The same shower, long continued; near α *Pegasi*.
62	July 6–17	334	+43	6	The *Lacertids*. The real centre for this shower is at 334°+48°.
	August 3–16	335	+45	18	Very active in August; meteors very swift.
63	July 6–17	47	+45	5	Apparently a very long-continued shower, near α *Persei*, and quite distinct to the *Perseids* of August 10 near η.
	August 3–16	46	+45	6	
	September 4–16	47	+45	7	= Comet of 1764, 49°+45°·5, July 25.
64	July 6–17	36	+47	6	A closely bordering shower to the last at θ *Persei*. = Comet of 770, 39°+45°, July 8.
65	August 3–7	40	+56	20	*Perseids*. Meteors very swift, invariably with streaks. The four positions are exact, and probably show a distinct advancement of the radiant in R.A. Horary number from 43°+58° on 10th, 57.
	August 10	43	+58	285	
	August 12	50	+55	14	
	August 16	60	+59	5	

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66	July 6–17	280+57	5	*Draconids.* Seen by many other observers in July and August.
67	July 6–17	285−12	5	Well-marked shower; meteors very slow and bright.
	August 3–16	283− 9	3	Uncertain and feeble in August. Greg (204), 285°−13°, June 24—July 6.
68	July 6–17	298− 8	5	Uncertain. Meteors slow. Schmidt, 294°−7°, June, July, August.
69	July 6–17	258+37	6	Exact. Seen by Prof. Herschel, July 16, 1871, at 257°+36°.
70	July 6–17	313+33	5	Not exact. Meteors very slow. *Cygnids* II. Greg (101), 315°+30°, July–Aug.
71	July 6—August 16	330+70	13	Short, swift meteors; near β *Cephei*; seen chiefly on August 12.
72	July 6–17	319+ 9	?	Rather uncertain. Feeble. Greg (118), 318°+7°, July 5—August 31.
73	August 3–16	40+28	8	Meteors rapid. Tupman and Schmidt, 39°+28°, August 22–28.
74	September 16	50+31	4	Meteors slow. Heis, 48°+35°, Sept.—Oct. 15. Distinct to preceding.
75	August 3–16	22+46	7	Maximum August 4. A new shower at γ *Andromedæ*.
	September 4–16	28+45	4	Doubtful in September. 22°+45°, September 5–12, Zezioli's observations.
76	August 3–16	330+10	12	Active shower; radiation somewhat diffuse.
77	August 10–12	70+65	10	A well-marked new shower in *Camelopardalis*.
78	August 3–16	18+63	15	*Cassiopeids.* Meteors swift and slow. ? two showers here.
79	September 4–16	16+54	8	These showers are probably distinct to the preceding.
	October 2–19	21+55	5	Confirms Greg (173), 14°+57½°, Sept. 15–Nov. 15. Heis, 21°+55°, Oct.
80	August 3–16	306+6	4	*Aquilids.* Slow meteors. Feeble shower in 1877.
81	July 28—August 7	147+62	3	Not exact. Fine, slow meteors. Schmidt, 148°+67°, August 8–17.
82	August 12–16	31+18	11	Strong maximum on August 12; meteors with streaks; a new A.M. shower.
83	August 3–16	61+48	11	A well-marked shower; exact; maximum August 12–16; suspected also in September (5 meteors).
84	August 3–16	287+45	3	Feeble and uncertain. G. & H., 285°+44°, August 2—September 25.

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No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
85	August 3–16	307	+67	5	Well seen by Corder on August 17, 307° + 65° (11 meteors).
	September 4–16	314	+66	5	Previous shower continued; not certain. Heis, 311° + 65°, Sept. 16–30.
86	July 6–17	295	+85	6	A series of showers not far from *Polaris*. In July–August, at 307° + 82°.
	August 3–16	320	+80	5	= Comet III. 1853. 300° + 80°, August 13.
	September 4–16	330	+78	4	Feeble and uncertain in September.
	October 2–20	310	+77	17	Active shower, with maximum on October 3–4. Meteors rather slow.
87	September 12	10	+ 0	3	Not very certain. Seen only on September 12.
88	September 4–16	242	+46	8	Radiant diffuse and inexact.
89	September 15, 16	88	+17	8	Beginning of the *Orionids*? A new A.M. shower.
	October 15–20	92	+15	57	*Orionids*. Maximum October 19, A.M. Very swift meteors; 47 had streaks.
90	September 15, 16	156	+41	5	Exact. Meteors swift, with streaks; just before daybreak.
	October 2–18	153	+42	4	Exact. A new radiant close to μ *Ursæ Majoris*.
91	August 3–16	30	+36	13	Exact. Active shower at β *Trianguli* of short, quick meteors.
92	September 4–16	160	+78	6	Not very exact. Swift, white meteors.
	October 2–18	167	+75	5	Meteors with streaks; a feeble shower.
93	September 4–16	267	+20	4	Not certain. Meteors very slow. Schmidt, 264° + 25°, July 5–31.
94	September 4–16	220	+78	10	A well-defined and active shower in *Ursa Minor*.
	October 2–18	213	+79	8	Meteors swift; less well defined in October than in September.
95	September 7–16	87	+34	10	Very swift meteors, with streaks. Maximum September 16. At θ *Aurigæ*.
96	September 4–16	61	+36	15	Exact. A rich shower; meteors very rapid, with streaks. Max. Sept. 7–16.

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97	September 4–16	100 + 58	10	Probably one long-enduring radiant, at 103° + 52°.
	October 2–19	105 + 50	15	Very exact in October. Meteors short and very rapid.
	October 28—November 13	103 + 48	21	An active shower; possibly new (105° + 50° and 98° + 45°).
98	September 4–16	255 + 65	6	Slow meteors; radiant fairly well defined, near *ζ Draconis*.
	October 2–5	253 + 64	4	
99	September 15–16	101 + 11	5	Slightly seen, and not very certain. Perhaps connected with following.
	October 2–19	103 + 12	22	Very rich shower; maximum October 8; meteors very rapid.
	October 28—November 13	107 + 11	11	Confirms Tupman, 107° + 12°, October 8, 1869.
100	September 4–16	284 + 62	4	Uncertain. Heis, 286° + 57°, September 16–30.
101	September 4–16	53 + 64	6	Chiefly September 5–7. Heis, 57° + 61°, October 1–15.
102	September 4–16	73 + 14	7	Meteors rapid; maximum Sept. 15. Tupman, 75° + 15°, Sept. 22, 1871.
103	September 4	40 + 72	6	An active transient shower. G. & H., 44° + 73°, September.
104	September 4–16	59 + 9	5	Meteors rapid, with streaks. Slightly seen and uncertain.
	October 28–November 13	62 + 9	6	Meteors rather slow; streaks. Schmidt, 62° + 6°, October 10–22.
105	September 7–16	49 + 14	6	Meteors rapid, with streaks. Schmidt, 51° + 14°, September 3–30.
	October 2–19	47 + 16	5	Confirmed by Corder at 47° + 14°, October 1877.
106	September 4–16	113 + 24	4	Slightly seen and rather uncertain. Beginning of *Gemellids*.
	October 2–19	115 + 29	13	Exact. Meteors rapid, with streaks. S. & Z., 113° + 29°, Oct. 21–25, 1868 (56 meteors).
	October 28–November 13	110 + 25	8	Exact. Meteors rapid; maximum on November 7. *Gemellids*.
107	September 4–16	70 + 85	7	Meteors rapid; radiant near *Polaris*.
	October 2–18	50 + 85	9	Confirms Greg $\binom{128}{143}$, 105° + 86°, September—October.

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No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
108	October 2–19	108	+38	16	Meteors rapid, with streaks; suspected also in September.
109	September 4–16	85	+53	8	Maximum September 5. *Aurigids.* Radiant not very exact.
	October 2–19	84	+55	18	Maximum October 5–8; two showers in October? suspected also in November.
110	September 15	130	+46	4	Meteors rapid, with streaks; seen just before daybreak.
	October 2–19	130	+47	11	Active shower; maximum October 15, 16; seen by several other observers.
111	September 16–October 7	20	+ 8	8	Not well seen; maximum September 16; meteors very slow.
112	September 4–16	72	+41	6	Meteors rapid. Confirms Corder, 75°+45°, October (14 meteors).
113	October 2–18	133	+79	22	Exact. Extremely well marked and certain shower, with maximum about October 3–4. Meteors very rapid, with streaks. = Comet I. 1825, 133°+77°, October 7.
114	October 2–18	138	+58	10	New shower of rapid meteors in *Ursa*.
115	October 2	225	+52	7	Exact. Fine shower of slow, bright meteors; new and rich radiant.
116	October 16, 17	178	+34	4	Exact. Short, quick meteors; new. At dawn.
117	October 2–19	100	+73	9	Long-continued and rich shower of very swift meteors in *Camelopardalis*.
	October 28—November 13	102	+73	14	Exact. Strong max. on Nov. 8, A.M. Seen by S. & Z. and Gruber on Oct. 28.
118	October 15–19	133	+21	18	Exact. Maximum Oct. 14–15. A shower of very, very swift meteors.
	October 28—November 13	127	+17	12	Exact. Maximum November 12–13. Long paths, not often with streaks.
119	October 15–18	120	+15	9	Exact. Distinct to preceding; meteors very rapid.
120	October 15–18	140	+28	9	Exact. Meteors with streaks; one meteor stationary. Near ϵ *Leonis*.
121	October 2–18	186	+71	6	Exact. Very feeble shower; meteors small, rather slow. Compare No. 157.
122	October 8	47	+28	8	Short, quick meteors. Seen only on October 8.
	October 28—November 13	43	+22	31	Exact. A very rich shower of rather slow meteors; max. Oct. 28—Nov. 1.
123	October 2–18	61	+47	8	Maximum October 8. Bright meteors; very rapid, without streaks.
	October 28—November 13	61	+49	8	Maximum November 4. Continuation of preceding.

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124	October 2–18	31 + 39	19	Active shower. Strong maximum on October 8. Short meteors, not very swift.
125	October 2—November 13	8 + 35	7	Rapid. Chiefly on October 8. Meteors swift.
126	October 2–18	77 + 31	9	Exact. Max. Oct. 8. Meteors with streaks. Tupman, 77° + 30°, Oct. 13.
	October 28—November 13	77 + 32	8	Exact. Maximum November 7. Meteors without streaks.
127	October 2–19	133 + 68	8	Meteors rapid. Possibly a new shower.
128	September—October 18	42 + 59	9	*Perseïds.* Not very certain. Confirms Greg (168), 46° + 55°, November.
	October 28—November 13	45 + 60	6	
129	October 2–5	290 + 70	7	Exact. Meteors rather slow. Confirms Heis.
130	October 5–8	56 + 52	4	Rather slow meteors. = Comet VI. 1847, 54° + 52°·5, October 4?
131	October 15–19	121 ± 0	6	Meteors very swift; A.M. showers. Tupman, 124° + 4°, November 7.
132	October 15–19	100 + 1	5	Tupman, 101° ± 0°, October 16.
133	October 17–19	63 + 22	7	The *Taurids* I. Meteors rather slow.
	October 28—November 13	57 + 18	9	A somewhat feeble shower in 1877. Average radiant position 60° + 20°.
134	October 8—November 13	32 + 50	8	Meteors slow. Seen chiefly on October 8. New radiant at *θ Persei.*
135	October 15–19	86 + 8	7	A subradiant of the *Orionids.* Tupman, 86° + 6°, October 8.
136	October 2—November 13	80 + 21	9	Meteors very swift. Radiant rather uncertain near *ζ Tauri.* Compare No. 152.
137	October 2—November 13	4 + 50	7	Inexact. Confirms G. & H., 5° + 55°, Oct., and Schmidt, 5° + 53°, Oct. 22–28. A radiant also strongly suspected at 11° + 70°, October 8–14.
138	October 28—November 13	30 + 16	11	Not very swift meteors. Greg (154), 34° + 23°, October 19—November 20.
139	October 28—November 13	133 + 31	10	Very swift meteors with streaks. Maximum November 10–12.
	November 25—December 13	131 + 32	6	Suspected also in September. Maximum December 2.
140	October 28—November 13	60 + 34	12	Meteors rather slow. Suspected at 60° + 37°, November 25—December 13.
141	October 28—November 13	19 + 30	7	Meteors rather slow. Radiant fairly exact. Gruber, 20° + 26°, Oct. 17–22.
142	October 28—November 13	25 + 46	4	Confirms Heis, 24° + 42°, November 1–15. Perhaps distinct to *Andromedes.*
	November 25–27	24 + 45	9	*Andromedes.* Meteors very slow. Maximum Nov. 25; very feeble on Nov. 27.

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No.	Date.	Radiant Point. R.A.	Dec. N.	No. of Meteors.	Notes and Comparisons.
1877.	1877.	°	°		
143	October 28—November 13	40	+77	9	Exact. New shower of rather slow meteors. Maximum October 31—November 4. Suspected in December also.
144	October 28—November 13	338	+59	6	Maximum Nov. 10. Confirms Heis, 338° + 56°, October 17—November 15.
145	October 28—November 13	97	+25	8	Rapid meteors. Maximum November 4. = *Gemellids* No. 106?
146	October 28—December 13	173	+11	5	New A.M. shower near *β Leonis*. Meteors very rapid.
147	October 28—November 13	40	+10	5	Maximum November 9. Confirms Tupman, 43° + 11°, October 13.
148	November 12	125	+40	5	Exact. One meteor stationary.
149	November 10–13	148	+24	5	A feeble return of the *Leonids*. Meteors very rapid, with bright white streaks.
150	October 28—November 13	70	+65	6	Exact. Maximum November 13, A.M. Well seen in 1876 at 69° + 66°.
151	December 1–13	109	+33	9	*Geminids*. Rapid, short meteors.
152	November 25–December 13	80	+25	6	*Taurids* II. Meteors very slow. Well seen in 1876. Sawyer, 80° + 22°, Dec. '77.
153	December 8–12	145	+ 7	8	Exact. Strong maximum Dec. 8, A.M. Meteors very swift, with streaks.
154	December 10–13	199	+19	5	Short, swift meteors. Just before daybreak. Slightly seen.
155	December 10–13	221	+43	3	A morning shower at *β Hercules*. S. & Z., 220° + 40°, January 19.
	January 4–9	221	+39	6	Meteors rapid, with streaks.
156	December 10–13	230	+33	3	Short, quick meteors. Just before daybreak. Slightly seen.
157	November 25–December 13	185	+71	9	A strong shower; maximum November 29. Rapid meteors with streaks.
158	November 25–December 13	70	+15	5	Rather uncertain. Meteors not very swift.
159	November 27	54	+48	4	Meteors swift; fairly exact. Seen while watching for *Andromedes*. Probably a rich shower. Weiss, 52° + 49°, Novovember 27, 1872.
160	November 27–December 13	87	+37	5	Diffuse and rather uncertain. Heis, 95° + 33°, December 1–15.
161	November 25–December 13	102	+45	4	Uncertain. Different to *Geminids*.
162	December 10–12	166	± 0	3	Meteors very rapid, with streaks. Only slightly seen.

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Radiant Points deduced from the Paths of 4,143 Shooting Stars observed by the Members of the Italian Meteoric Association in the year 1872. By W. F. Denning, Esq.

The radiant points given in the table which follows were determined by the projection of the meteors in the Catalogue entitled *Osservazioni di stelle cadenti fatte dai membri dell' Associazione Meteorica Italiana durante l'anno 1872.* (Milan 1874). The book contains 7,512 paths in all, numbered progressively; but of these no less than 3,621 (Nos. 2684 to 6304) are for August 4–13, and chiefly *Perseïds*, which it was not considered essential to examine thoroughly. The number of meteors projected was 4,143, and the radiant points deduced therefrom 315, of which, however, a large number are duplicates of the same showers. Less than 200 distinct systems are probably shown. The meteors examined and resulting radiants for each month were as follow:—

No. in Table.	Period of Observations.	Length in days.	Meteors Projected.	Numbers in Catalogue.	Radiants Reduced.
I.	Jan. 1–15	15	313	1 to 313	30
II.	Feb. 1—Mar. 12	42	381	344 „ 724	24
III.	Mar. 31—April 12	13	542	731 „ 1272	37
IV.	May 3–15	13	269	1319 „ 1587	22
V.	May 26—June 13	19	356	1588 „ 1943	31
VI.	June 26—July 11	16	459	1961 „ 2419	36
VII.	July 15—Aug. 2	18	264	2420 „ 2683	27
VIII.	Aug. 6–12 (A.M.)	7	413	2900 „ 6274	25
IX.	Aug. 24—Sept. 14	22	455	6307 „ 6761	36
X.	Oct. 29—Nov. 13	16	255	6820 „ 7074	24
XI.	Nov. 25 to Dec. 31	37	436	7077 „ 7512	23

Thus the observations (and reductions) are broken up into periods of from about 13 to 22 days. The intervening voids were caused by moonlight. In two cases (Feb.–March and Nov.–Dec.) I incorporated two following sets of observations, because the radiants were generally the same, and the meteors observed, if taken singly, insufficient to indicate many of the positions accurately. Only 413 of the large number between August 4–13 were projected. These were seen in the mornings, and nearly all of them selected as probably belonging to radiants eastwards of the *Perseïds*, and perhaps to new showers or to showers feebly observed before. There are no reductions for October, only 37 meteors having been observed during that month. The tracks were pencilled upon the star charts prepared by the Luminous Meteor Committee of the British Association for the use of their observers. Some difficulty was occasionally

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found in getting at the radiant points, for the shooting stars recorded during several of the periods presented singular discordances in their directions, and were a very confused mass, requiring much care in assigning the true centres. In such cases it is obviously very important to consider the length of path and observed velocity of the individual meteors. The difficulty mentioned in sorting them arose not so much from errors of observation as from the large number of showers in contemporaneous operation, and the closely bordering positions of some of them.

The majority of the radiants in the table agree with showers previously discovered and included in the catalogues of Greg and Herschel, Heis, Schmidt, Schiaparelli and Zezioli, Tupman, and others, and accord in many instances with the resulting radiants from my own observations of shooting stars during the last two years. A proportion of the showers are new, and a few of them apparently rich systems (agreeing in several cases with cometary dates and radiants), which it will be desirable to re-observe and confirm in future years.

The chief radiant points observed were the *Perseïds* (August 1–13) and *Andromedes* (November 27). A number of minor systems were also fairly active; and to what extent may be found by a reference to the table, in which I have given the number of meteors conforming to each radiant; but the figures relating to this detail should not be considered exact. In cases where a meteor converged on two or more radiants in the same line it may have been counted twice over, and when far away from the radiant one will sometimes have been omitted altogether. Precision in this matter is not very important.

Many of the showers endured apparently for much longer periods than is usually attributed to them. Thus the *Perseïds* (or a succession of coincident radiants in *Perseus*) continued in feeble action during the whole of the last five months of the year, and the *Coronaids* were seen between March and July. Many additional samples of long duration will be found on inspecting the table, and my own observations are strongly corroborative in this particular. It appears certain that shooting stars continue to fall, and sometimes abundantly, from radiant points long after the first and maximum display, which usually occurs when the radiant is near the apex of the Earth's way.

In going over this Italian catalogue a few observed peculiarities struck me which may be worthy of note here. On May 3 Signor Maggi, at Volpeglino (who appears to have been one of the most successful of the observers), saw three meteors in succession with identical paths. They were of the 2nd mag., and very swift, the observed tracks being from 238°+78° to 16°+88°, and belonging to the radiant No. 11, IV., in the table, from which others were seen on the same night. The same observer recorded on Aug. 10, $11^h\ 19^m$ to $15^h\ 19^m$, five shooting stars also with exactly similar paths, namely, from 91°+59° to 101°+59° (*see* Nos. 4971, 4981, 5062, 5080, 5146).

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Another was seen on the same night from 91°+59° to 98°+60° (No. 5102). These foreshortened paths conform to the radiant in *Auriga*, No. 10, VIII. Nos. 4997, 4998, and 5007 supply another instance of similar paths (15°+35° to 1°+14°, *β Andromedæ* to *γ Pegasi*), and belonged to the *Perseïds*. Not many stationary meteors were registered during the year. One at 76°+46° (No. 5017 1st mag.), on August 10, strongly supports the shower No. 11, VIII. Another at 33°+33° (No. 3001), on August 5, agrees with No. 19, VIII. A fireball, comparable to the Moon, was seen stationary by Maggi on August 25, at 10°+7° (No. 6319). He also noted a meteor at 48°+49° (No. 5036), without movement, on August 10. The latter observation is valuable as confirmation of the radiant No. 14, VIII., in *Perseus*, which is a well-marked shower in August, and quite distinct to the ordinary *Perseïds*, at about 44°+56°. Other stationary meteors seen registered as follows: at 146°+9°, on April 3 (No. 926); at 163°+58°, on May 9 (No. 1512); at 191°+47°, on May 15 (No. 1586); and at 215°+16°, on July 3 (No. 2186).

The length of the meteor paths, as given in this catalogue, is, I believe, greater as a rule than the average in the lists of Zezioli, Heis, or Tupman, a large number of which I have recently been engaged in examining.

So far as can be judged by comparing the resulting radiants with those obtained by previous observers, and more lately by myself, the meteors in this Italian catalogue appear to have been very carefully and accurately registered.

Ashleydown, Bristol,
January 1878.

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Radiant Points of Shooting Stars, deduced from Observations by the Members of the Italian Meteoric Association in the year 1872. By W. F. Denning.

No. of Radiant.	I. Jan. 1–15. 313 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	II. Feb. 1–March 12. 381 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	III. March 31–April 12. 542 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	IV. May 3–15. 269 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	V. May 26–June 13. 356 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	VI. June 26–July 11. 459 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	VII. July 15–August 2. 264 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	VIII. August 6–12 A.M. 413 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	IX. August 24–Sept. 14. 455 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	X. Oct. 29–Nov. 13. 255 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	XI. Nov. 25–Dec. 31. 436 Meteors. Radiant. R.A. °	Dec. °	No. of ←°.	No. of Radiant.
1	184	+52	6	182	+56	20	209	+ 6	25	202	+ 7	14	201	+11	9	53	+62	7	47	+57	6	44	+56	—	45	+57	12	47	+56	4	46	+57	7	1
2	109	+34	8	105	+30	12	201	+18	15	202	+17	17	293	+14	22	290	+15	20	291	+17	5	100	+71	38	339	+46	24	100	+71	5	132	+76	8	2
3	278	+35	9	180	+36	12	184	+35	15	186	+36	8	288	+55	15	279	+59	26	284	+60	9	283	+63	6	280	+60	9	280	+70	5	285	+79	5	3
4	80	+23	11	83	+24	15	228	− 5	9	229	− 4	18	273	− 3	11	273	− 2	20	272	+ 4	7	61	+39	35	67	+35	10	62	+36	10	62	+38	9	4
5	164	+35	5	158	+28	21	162	+34	13	162	+34	10	312	+22	16	313	+20	7	320	+23	11	70	+64	31	64	+65	13	73	+65	9	68	+65	7	5
6	49	+61	7	53	+66	13	204	− 8	7	209	− 8	9	343	+16	8	344	+12	9	337	+14	10	50	+75	28	40	+73	10	45	+70	6	40	+70	8	6
7	230	+24	7	129	+22	19	139	+16	8	252	+47	8	250	+45	5	250	+48	10	349	− 1	6	50	+85	4	68	+84	15	54	+ 6	10	79	+57	7	7
8	155	+64	10	145	+55	5	157	+45	12	150	+45	7	257	+30	14	255	+37	12	256	+37	11	92	+56	18	100	+56	10	100	+60	6	265	+65	4	8
9	104	+53	13	133	+67	10	133	+66	20	135	+68	10	233	+60	15	250	+62	18	233	+68	7	76	+33	21	78	+23	8	78	+37	7	245	+65	4	9
10	228	+53	8	260	+66	6	258	+60	20	315	+55	6	312	+63	5	316	+51	13	318	+49	9	81	+57	17	315	+47	9	189	+57	8	180	+64	3	10
11	221	+43	7	111	+62	22	232	+54	17	233	+47	24	305	−10	8	303	+ 7	7	302	+ 7	4	75	+45	13	343	+ 9	11	147	+ 2	6	346	+47	7	11
12	241	+63	6	28	+35	16	170	+78	7	180	+79	6	285	± 0	8	15	+35	11	13	+29	12	4	+31	4	10	+34	20	9	+37	6	102	+48	5	12
13	100	+42	6	101	+44	12	106	+52	10	175	+26	6	353	+39	7	8	+54	6	8	+54	15	52	+20	11	302	+ 9	9	62	+20	13	64	+15	7	13
14	104	+14	7	177	+10	32	179	+ 9	15	170	−10	9	35	+47	7	163	+59	9	167	+55	5	49	+47	11	44	+43	7	157	+17	8	26	+46	202	14
15	204	+45	8	205	+51	7	206	+44	30	210	+48	18	215	+55	17	216	+47	10	351	+53	11	45	+24	8	45	+20	8	45	+26	7	48	+32	7	15

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16	140+77	8	147+ 4	17	147+ 1	7	203+39	9	10+80	6	64+46	10	65+51	12	60+48	22	63+48	12	62+47	10	105+14	6	16
17	40+77	8	300+80	12	215+83	10	300+79	7	279+77	10	290+82	8	315+60	10	87+34	8	180+75	10	135+21	12	330+66	6	17
18	65+65	10	77+60	9	235+25	19	241+26	9	240+21	9	248+22	16	30+47	10	34+50	3	286+45	5	160+40	8	170+84	5	18
19	140+30	6	210+70	14	210+66	22	218+62	7	215+70	7	210+62	7	23+37	7	33+33	7	34+35	14	130+45	8	110+35	10	19
20	150+43	7	10+60	8	83+62	5	247+57	8	5+17	5	335+26	10	337+28	9	347+36	4	140+76	5	180+32	6	24+27	4	20
21	245+72	7	106± 0	5	265+38	31	265+38	15	278−35	5	291+45	8	294+40	10	47+32	9	310+67	12	157+30	4	120+40	5	21
22	258+45	5	55+19	8	229+35	6	251+ 8	10	260+ 8	9	261+11	14	294− 7	6	135+66	8	252+36	5	140+66	7	125+65	4	22
23	120+46	8	123+46	5	231+ 7	17			238+13	8	235+ 7	14	287+27	10	87+15	5	17+61	23	136+11	5	20+36	7	23
24	195+63	5	288+12	6	277+10	7			192+35	7	188+29	9	260+63	5	17+ 5	5	300+82	14	88+ 7	7			24
25	120+15	6			216+14	8			214+16	12	215+16	7	285+44	8	5+20	6	24+44	7					25
26	157+13	6			338+61	18			337+59	10	325+66	8	338+64	6			342+59	11					26
27	135+48	6			105− 6	6			258−14	19	269−13	12	330+42	6			358+20	11					27
28	168+ 3	5			190+20	12			248− 3	7	253− 5	7					235+65	9					28
29	200+58	7			223+23	14			286+36	15	278+35	29					329+ 9	5					29
30	110+70	8			262± 0	10			332± 0	8	325+ 3	9					15+10	10					30
31					255+27	9			315+32	22	315+33	15					270+20	7					31
32					304+12	9					24+61	10					240+25	6					32
33					151+23	7					290+70	11					333+27	12					33
34					278+46	8					320−12	6					280+30	6					34
35					287+27	5					344+46	13					260+50	5					35
36					91+58	6					27+50	7					216+49	5					36
37					183+62	7											240+46	4					37

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On the Determination of the Axial Position of Mars with respect to the Earth at any Epoch. By Richard A. Proctor, Esq.

Two or three months ago I began the work of re-examining the chart of *Mars* which I formed in 1867, with the view of introducing some of the corrections suggested (1) by the observations of Messrs. Green and Knobel during the opposition of 1873, and (2) still more by the remarkably fine series of drawings made by Mr. Green during the opposition of last autumn. But hearing that many valuable observations had been made by Mr. Trouvelot with the Washington refractor, and by Schiaparelli, at Milan, and having also a wish to go systematically through some of the earlier observations, which are inadequately represented in my present chart, I found it impossible to complete the new chart so soon as I had expected. In fact, I may have to defer the communication of my results to the Society for some few months. In the meantime, however, it occurred to me that it might be well to mention what I proposed to do; in order that any observer, who had drawings of interest and might wish to see the results (if new) incorporated in the proposed chart, might have an opportunity of communicating with me, and perhaps sending tracings of such drawings. It also seemed to me that, as there are many who have drawings which they may wish to compare with the calculated actual aspect of the planet at the epochs when those drawings were made, it might be useful to indicate a comparatively simple construction by which the projection of *Mars* at any epoch may be obtained if the declination and right ascension of the planet be known at the time of observation. The last figure (fig. 4) of the present paper indicates the actual construction for two cases, the light lines representing the part of the construction common to all cases, the broken light lines representing the construction for one case, the dotted lines that for the other case, the heavy lines representing the deduced position of the axis and equator of *Mars* for the first case dealt with. It is not at all necessary in the actual employment of this construction that the explanation illustrated by the first three figures should be read through; fig. 4 and the letterpress relating to it will suffice amply for that purpose. But for the sake of completeness, and for those students of such matters who may desire to recognise the reasons on which the construction is based, and one way at any rate by which the usual formulæ for such cases can be established, I give some preliminary explanations.

In the first place let it be noted that by a simple construction we can obtain the position of the pole and equator of *Mars* with respect to the pole and equator of our heavens. Thus we have, from Oudemans' elements, which may be regarded as relating to the year 1834—

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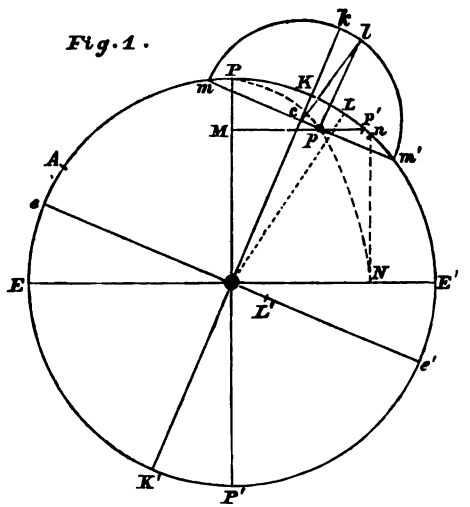
Longitude of pole of *Mars* = 349° 1′
Latitude = 61 9
} for Ecliptic, 1834·0.

Now in fig. 1 (as in the others) let POP′ represent the polar axis of the heavens, EOE′ the equator projected into a straight line. In fig. 1 (only) *e*O*e*′ is the ecliptic similarly projected, the ascending node (♈) being at O on the concavity, the descending node at O on the convexity of the sphere PP′; KOK′ the ecliptical polar axis. Thus POP′ is the projection of the equinoctial colure. Let KAK′L be an arc of 349° 1′, or KL an arc of 10° 59′; then a parallel LL′ to KO would give L′ the point on which a great circle through KK′ and the pole of *Mars* on the heavens would intersect the ecliptic. Also if K*m*, K*m*′ are arcs of 28° 51′, the straight line *mcm*′ represents the projection of a small circle having K as nearest pole, and passing

Fig. 1.



through the pole of *Mars*. The point *p* occupied by this pole might obviously be obtained by dividing *cm*′ in *p*, so that

$$cp \cdot cm' :: \mathrm{OL}' : \mathrm{O}e';$$

for then *p* would lie on an ellipse having KOK′ as major axis, and OL′ as semi-minor axis—this ellipse being the projection of the great circle through KL′K′. But the best way to obtain the point *p* is to draw the semicircle *mkm*′ on *mm*′, *cl* parallel to OL, and *lp* perpendicular to *mm*′. Or practically the best way is with a protractor to make the angle K*cl* (or *kcl*) equal to 10° 59′, and drop perpendicular *lp* to *mm*′.

Having thus obtained *p*, the northern pole of *Mars* on the heavens, we have only to draw M*pp*′ perpendicular to PP′ to obtain the arc P*p*′, which is the north polar distance (I) of the pole of

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Mars. The right ascension of p is obtained by dividing OE′ in N in the same proportion that Mp′ is divided in p, and drawing Nn perpendicular to OE′; for then an ellipse having PP′ as major axis and passing through p will pass through N and be the projection of an hour-circle through p, and the inclination of this circle to the equinoctial colure POP′ is measured by the arc Pn. The right ascension of p is, however, equal to the arc PEP′n, and as the ascending node of the equator of *Mars* on the Earth's equator is 90° in advance of the pole of *Mars*, in right ascension, it follows that the arc E′n measures the right ascension, N, of the ascending node of *Mars* on the Earth's equator. This construction carefully made will give results very close to those obtained by the ordinary formulæ of spherical trigonometry, viz.:—

$$\left.\begin{array}{l}\text{R.A. of ascending node of } \sigma\text{'s equator on } \oplus\text{'s equator (N)} = n\text{E}' = 47^\circ\ 33'{\cdot}9 \\ \text{Inclination of } \sigma\text{'s equator to } \oplus\text{'s equator (I)} = \text{P}p' = 39^\circ\ 55'{\cdot}6\end{array}\right\} \text{ in 1834.}$$

To obtain N and I for any other date, $1834 + t$, we make the same correction precisely as for a star in same R.A. and Decl., that is, we correct for the effects of precession, getting for $1834{\cdot}0 + t$

$$\text{N} = 47^\circ\ 33'{\cdot}9 + 0'{\cdot}50\,t,$$
$$\text{I} = 39^\circ\ 55'{\cdot}6 - 0'{\cdot}23\,t,$$

with amply sufficient approximation.

In passing I may note that the spherical formulæ for obtaining N and I from λ, the longitude of ascending node of equator of *Mars* on ecliptic, and i, the inclination of equator of *Mars* to ecliptic, can be readily deduced from the constructions used in fig. 1. For regarding Mp and OM as projections of Oc and cp (and remembering that angle POK $= \omega$, the obliquity of the ecliptic) we have (putting PO equal to unity),

$$\cos \text{P}p' = \text{OM} = \sin i \cos \omega - \cos i \sin \omega \cos \lambda,$$
$$\text{M}p' \cos \text{N} = \text{M}p = \cos i \sin \omega + \sin i \cos \omega \cos \lambda,$$

that is,

$$\cos \text{I} = \sin i \cos \omega - \cos i \sin \omega \cos \lambda,$$

and

$$\sin \text{I} \cos \text{N} = \cos i \sin \omega + \sin i \cos \omega \sin \lambda,$$

the required equations.

Let us next consider the construction for obtaining the projections of *Mars* from his known declination and right ascension at any epoch, N and I being known as obtained above, or otherwise.

In fig. 2, POP′ being the polar axis of the Earth, EOE′ the projection of the celestial equator, let pOp' be the polar axis of

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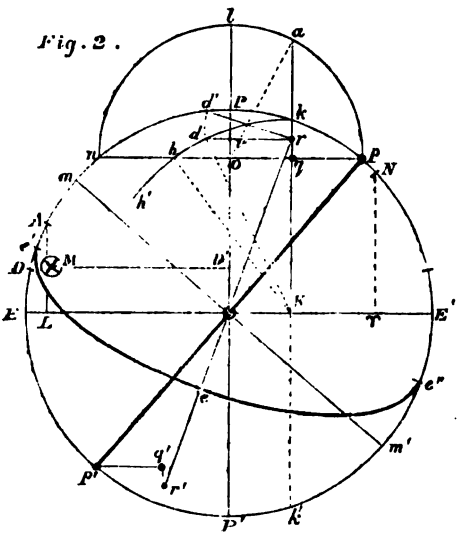
Mars, mOm' the projection of the equator of *Mars*, the ascending node on the Earth's equator being on the concavity of the sphere PP′, at O. Then, if we take arc PN = N, and draw N♈ perpendicular to OE′, ♈ on the concavity of sphere PP′ is the first point of *Aries*. Now, let

R.A. of δ at epoch = α,

N. Decl.„ „ = δ.

Then, if arc $pPA = \alpha$, and arc $ED = \delta$, and AL, DD′ be drawn perpendicular to EO and PO respectively, *Mars* lies at M, the intersection of DOD′ with an ellipse having PO, OL as semi-axes. The polar axis and equator of *Mars's* globe would of

Fig. 2.



course be situated, in the small disk M, precisely as pOp' and mOm', the polar axis and equator of the Martian heavens, are situated in the disk PP′. And clearly, if we wish to ascertain how *Mars* at M would appear as seen from the Earth at O, we have only to suppose the sphere PEP′ so turned that M shall be brought to O; then the sphere PEP′ will be presented in precisely the same aspect as the globe of *Mars*, for the Martian celestial axis and equator, pp' and mm', are parallel to the axis and equator plane of the globe *Mars* throughout the supposed turning motion. Now, so far as the shape assumed by the equator mm' is concerned, the turning might be effected in any way we pleased so long as the centre of M was finally brought to coincidence with O. But as we wish to determine also the position of the polar axis of *Mars* with respect to the meridian, it is convenient to turn the sphere PEP′ by two motions which leave the axis

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PP′ (or rather its projection) unchanged in position. Thus we first turn the sphere about the axis PP′, bringing M to D′, and carrying the Martian pole p to q (on the convexity of the sphere PP′ in this case, since MD′ lies on the concavity of that sphere); and then, secondly, we rotate the sphere about EOE′ till the centre of the globe M is brought from D′ to O, and the Martian pole from q to r. Then is rOr' the polar axis of *Mars*, Or' the remoter half, and the equator of *Mars* has the form shown by the half ellipse $e'ee''$, Oe, the minor axis, being (by a well-known property of orthogonal projection) equal to rd' drawn perpendicular to rOr'.

Now, let us consider what construction is indicated for determining the true positions of q and r.

The first rotation of the sphere, round PP′, is through an angle measured by the arc $PA = (N - \alpha)$, the right ascension of *Mars* from the ascending node of his equator on the Earth's. Thus, if we draw *pon* perpendicular to POP′, representing the projection of a declination parallel through p, and on the semicircle *pln* take arc *pa* measuring the angle $poa = (N - \alpha)$, aq perpendicular to *np* gives us the point q. Produce aq to k', giving kk' the projection of a small circle parallel to great circle POP′. Then we have to rotate the sphere PP′ through an angle measured by arc $DE = \delta$, the declination of *Mars* (north, positive). Hence, if khh' be part of semicircle on kk' cutting *np* in h, and the arc hd measures the angle $hKd = \delta$ (on side of h towards k, because obviously the rotation of D′ on concavity of sphere to O will carry q on convexity towards k), then dr perpendicular to kK gives the true position of the northern Martian pole at r; the southern will be at r', Or' on rO produced being equal to Or; q' shows the position of the southern pole at the end of the first rotation. Of course, both q' and r' are on the concavity of the sphere PP′ in the special case dealt with.

It may be noticed that

$$rd = rd';$$

for the square on $rd' =$ rectangle under kr, $rk' =$ square on rd. So that the construction gives $Oe\ (= rd')$ without occasion for drawing rd'.

Next let us consider the formulæ resulting from this construction. It will be convenient to regard the angle kKh as an auxiliary angle. Note also that

$$(hq)^2 = kq.qk' = nq.qp = (aq)^2;\ \text{or, } hk = aq.$$

What we require is first the angle POr; and next the angle between the plane $e'ee''$ and the line of sight to O, or the elevation of the Earth above the plane of the equator of *Mars*. The former angle is commonly regarded as positive when r is on the east of PP′, so that in the position of r in fig. 2 this angle is negative. The Earth's elevation above the plane of the equator

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of *Mars* is considered positive when the Earth is north of this plane, as in the case illustrated by fig. 2. Put then

$$\angle \mathrm{PO}r = -p$$

and

inclination of $e'e\,e''$ to line of sight to O $= l$.

Put also

auxiliary angle $k\mathrm{K}h = \mathrm{Q}$.

Then

$$\tan \mathrm{Q} = \frac{hq}{\mathrm{K}q} = \frac{aq}{\mathrm{K}q} = \frac{oa \sin aop}{\mathrm{O}o} = \frac{\sin \mathrm{I} \sin (\alpha - \mathrm{N})}{\cos \mathrm{I}},$$

$$\tan p = -\frac{\mathrm{OK}}{r\mathrm{K}} = -\frac{oq}{\mathrm{K}d \cos dk\mathrm{K}} = -\frac{aq \cot_\delta (\alpha - \mathrm{N})}{hq \operatorname{cosec} \mathrm{Q} \cos (\mathrm{Q} - \delta)} = -\frac{\cot (\alpha - \mathrm{N}) \sin \mathrm{Q}}{\cos (\mathrm{Q} - \delta)},$$

and

$$\tan l = \frac{d'r}{r\mathrm{O}} = \frac{dr}{r\mathrm{O}} = \frac{r\mathrm{K} \tan (\mathrm{Q} - \delta)}{r\mathrm{O}} = \cos p \tan (\mathrm{Q} - \delta).$$

These are the required formulæ for finding p and l.

We can also deduce from the construction the following formulæ :—

$$\cos l \sin p = -\sin \mathrm{I} \cos (\alpha - \mathrm{N}),$$

$$\cos l \cos p = \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \sin \delta + \cos \mathrm{I} \cos \delta,$$

$$\sin l = \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \cos \delta - \cos \mathrm{I} \sin \delta.$$

We note that

$$\sin \mathrm{I} \cos (\alpha - \mathrm{N}) = oa \cos aop = oq = or,$$

and

$$\sin \mathrm{I} \sin (\alpha - \mathrm{N}) = oa \cos aop = aq = hq;$$

also radius OP being throughout regarded as unity,

$$\cos l = \mathrm{O}r, \qquad \sin l = d'r = dr,$$

$$\cos l \sin p = -\mathrm{OK} = -or = -\sin \mathrm{I} \cos (\alpha - \mathrm{N}),$$

$\cos l \cos p = r\mathrm{K}$ = projection of hq and $q\mathrm{K}$ on a line through K inclined at an angle = $d\mathrm{K}h$ to $h\mathrm{K}$, towards Kh,

$$= hq \sin \delta + \mathrm{O}o \cos \delta$$

$$= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \sin \delta + \cos \mathrm{I} \cos \delta,$$

and

$\sin l = rd$ = projection of hq and $q\mathrm{K}$ on a line through K perpendicular to the one last mentioned,

$$= hq \cos \delta - \mathrm{O}o \sin \delta,$$

$$= \sin \mathrm{I} \sin (\alpha - \mathrm{N}) \cos \delta - \cos \mathrm{I} \sin \delta$$

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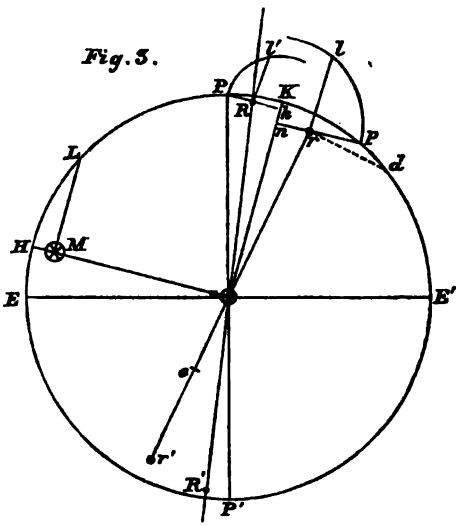
In some cases, where, for instance, the student may have had occasion to determine accurately the projection of the centre of M on the disk PEP′, it may be convenient to carry M to O by a single rotation. Fig. 3 illustrates this method. We join MO; draw ML and OK perpendicular to MO, and *pn*, P*k* to OK. Then taking arcs P*l*, P*l*′ about centres *n* and *k* respectively, each measuring the same angle as arc KL, we draw *lr* and *l*′R parallel to OK, giving *r*O*r*′ the position of north Martian pole, and ROR′ the

Fig. 3.



changed position of the polar axis POP′ corresponding to the rotation round KO. The angle ROr is the same as POr from the former construction and the minor semi-axis O*e*, the equator of *Mars*, is given by this construction as by the former, since (drawing *rd* perpendicular to O*r*)

$(rl)^2$ = rect. under segments on either side of *r*, of chord of which *pn* is part = $(rd)^2$;

that is

$$rl = rd = Oe.$$

The Actual Construction for Axis and Equator of Mars.

In fig. 4 the actual construction for two cases (with the full result for one only) is shown; and this part of the present Paper is all that the non-mathematical student of the planet need follow.

First, the circle EPE′P′ is drawn with diameters POP′, EOE′ at right angles to each other. Arcs P*p*, P*n* are taken on either

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side of P, each measuring an angle of $39\frac{1}{2}°$ (I); *pon* is drawn and the half-circle *nlp*; and P′P is produced to *l*.

So much is common to all cases. (This part of the construction may with advantage be carefully drawn in ink on card, to be traced off when wanted; though this will depend on the use to which the constructions are to be applied.) We may add a mark at N, taking $PN = 47\frac{1}{2}° = N$; but this mark need not actually be used, seeing that it is as convenient to take $(\alpha - N)$ at once.

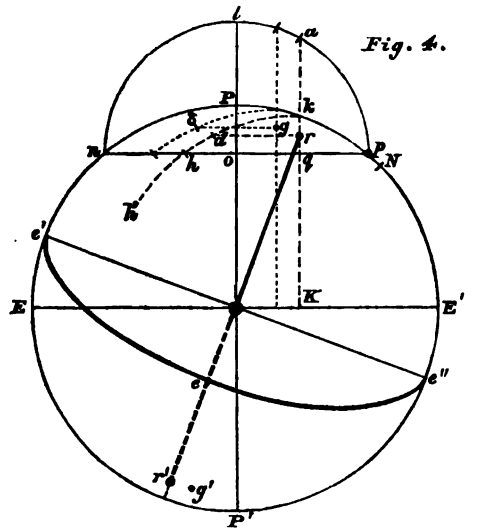

Fig. 4.

Now let the R.A. and Decl. of *Mars* at the epoch dealt with be α and δ respectively.

With a protractor take *pa* measuring angle $(\alpha - N)$ about *o*; and draw *aq*K perpendicular to *pn* and E′E. About K sweep out the arc *khh*′. With a protractor take *hd* measuring arc δ about K. Then *dr* perpendicular to *k*K gives *r* the north pole of *Mars*; *r*O*r*′ (O*r*′ = O*r*) is the polar axis; *e*O*e*′ is the major semi-axis of Martian equator; and O*s* (= *rd*) is the minor semi-axis.

N.B. If $(\alpha - N)$ is between 0° and 180°, *q* lies on the convexity of the sphere PP′. But if $(\alpha - N)$ is between 180° and 360°, we measure this arc from *p* to *n* and then from *n* back to *p* (to avoid drawing the lower semicircle) and *q* lies on the concavity of the sphere PP′. In the former case, northern declinations are measured from *h* towards *k*, and southern declinations from *h* towards *h*′. In the latter case northern declinations are measured from *h* towards *h*′, and southern from *h* towards *k*.

The construction for a second case, $(\alpha - N)$ a few degrees greater and δ also, is shown by the dotted lines. The resulting positions of the poles are shown at *g* and *g*′; and *g*δ is the resulting length of the minor semi-axis of the Martian equator. It is not

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necessary, in preparing for an approaching opposition of *Mars*, to make more than five, or at the most seven constructions, the middle one for the day of opposition, since the intermediate positions of the pole and equator of *Mars* can be readily estimated with amply sufficient exactness.

In a future paper I propose to make a few remarks (i) on the completion of the projection of a planet's disk after the position of the poles and equator have been determined; (ii) on the determination of the central areographic longitude for any given epoch; and (iii) on the determination of the centre of illumination.

On the Proper Motion in Right Ascension of η^2 Draconis.

By A. W. Downing, Esq.

My attention was attracted to this star by reading in the preface to the *Nautical Almanac* that the proper motion adopted was that given in Mädler's *Bradley*, and on looking further into the matter I find that from 1848 to 1873 the proper motion in R.A. used in the *Nautical Almanac* was $+0^s{\cdot}023$, this being the value given in the British Association Catalogue, and since that time $+0^s{\cdot}005$, taken from Mädler's *Bradley*, has been used. The British Association Catalogue value, $+0^s{\cdot}023$, has also been adopted in the Greenwich Catalogues since 1845, the Right Ascension of η^2 *Draconis* not being given in the *Fundamenta*, though the Declination is, and consequently the proper motion in the former element has not been computed by Mr. Main or Mr. Stone, whose determinations are based on the *Fundamenta*. The proper motion used in the American *Nautical Almanac* is $+0^s{\cdot}0024$. As I found that these three authorities differed, I determined to calculate the proper motion in R.A. afresh from the materials afforded by the best available Catalogues.

For this purpose I have, by the kind permission of the Astronomer Royal, used as an assumed R.A. for $1872{\cdot}0$ the place given in the Greenwich Nine-Year Catalogue, so that—supposing the proper motion $=0$—we have for any time

$$\alpha = 16^h\ 22^m\ 15^s{\cdot}668 + 0^s{\cdot}8017\ (t-1872) + 0^s{\cdot}0000939\ (t-1872)^2, \qquad (1)$$

and from this formula I have computed the R.A. for the epoch of each of the Catalogues used.

The following is a list of the Catalogues:-

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Catalogue	Epoch	Mean Year of Obs.	Weight
Piazzi	1800	1800	2
Groombridge	1810	1806·8	2
Struve (Cat. Gen.)	1830	1824	3
Pond	1830	1830	2
Madras (Taylor)	1835	1835	½
Armagh	1840	1832·6	½
Greenwich	1840	1839	4
Washington (Gilliss)	1840	1841·5	2
Radcliffe	1845	1841·9	3
Greenwich	1845	1844	4
Poulkova	1845	1849·7	6
Greenwich	1850	1851·1	4
Washington	1860	1852·5	2
Greenwich	1860	1858·1	4
Radcliffe	1860	1858·5	2
Greenwich	1864	1865·1	3

The places given in the various Catalogues have been reduced to Professor Newcomb's Standard (*Washington Observations for* 1870, *Appendix III.*), and have also been corrected for the part of the proper motion, if any, which has been applied to the observed place in reducing it to the epoch of the Catalogue. Then the differences between the Catalogue places thus corrected and the R.A.'s computed by means of formula (1) have been found; whence, by the method of least squares, we have the normal equations:—

$$44\,\Delta\alpha - 1291\,\mu = +2^s{\cdot}5,$$
$$-1291\,\Delta\alpha + 49354\,\mu = -26^s{\cdot}494,$$

where $\Delta\alpha$ is the correction to the assumed R.A. and μ is the proper motion in R.A. for 1872; therefore

$$\Delta\alpha = +0^s{\cdot}177,$$
$$\mu = +0^s{\cdot}00408,$$

and (1) becomes

$$\alpha = 16^h\,22^m\,15^s{\cdot}845 + 0^s{\cdot}8058\,(t-1872) + 0^s{\cdot}0000937\,(t-1872)^2$$

I may add that the values of the proper motion in N.P.D. given by the authorities cited above agree fairly well.

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On a New Variable in the Constellation Ara.

By J. Tebbutt, Esq.

I have recently been engaged in a re-examination and reduction of the stars compared by me with Comet III., 1862, with a view to the publication of my collected astronomical observations. The observations of that comet were made previously to the erection of my Observatory, and with a ring micrometer on my old telescope of 3¼ inches aperture, the instrument being mounted on an ordinary tripod without divided circles of any kind. The method which I adopted for the identification of the smaller comparison stars was to observe their transit, across either the ring or the field of the telescope, in conjunction with some star visible to the naked eye and differing but little from them in declination, the instrument, of course, remaining undisturbed in the interval. The bright star was then identified by sextant observations, and hence the approximate position of the small comparison star became known. It was in this way that I identified the stars *Brisbane* 5754 and 5799, compared with the comet on the evening of October 4, 1862, the bright star being about an hour of Right Ascension further east. The two stars were again compared with the bright one on the evenings of the 5th, 6th, and 9th, and the position of the latter carefully fixed on the last-mentioned date by means of sextant distances from θ *Scorpii*, *Altair*, *Antares*, and ϵ *Sagittarii*. These observations showed that the star was not to be found in the B. A. Catalogue, or even in the exhaustive one of Lacaille. My journal states that σ *Arœ* and this star were both visible to the naked eye, the latter being about a degree north-east of the former, and the brighter of the two. The journal also states that the star was of the fifth magnitude, and had a small round nebula to the north of it and in the same field of view. A careful reduction of the sextant observations from *Altair* and *Antares*, regard being had to mean refraction only, for temperature 50° and pressure 29·6 in., gives for the apparent place of the star on October 9, 1862, R.A. $= 17^h\ 29^m\ 6^s{\cdot}60$, N.P.D. $= 135°\ 23'\ 42''{\cdot}8$, which, reduced to the mean place for 1862·0, is R.A. $= 17^h\ 29^m\ 2^s{\cdot}51$, N.P.D. $= 135°\ 23'\ 33''{\cdot}8$. Adopting $+4^s{\cdot}415$ and $+2''{\cdot}69$ as the annual precession in R.A. and N.P.D. respectively, I get the following as the mean position for the beginning of 1878:—R.A. $= 17^h\ 30^m\ 13^s{\cdot}15$, N.P.D. $= 135°\ 24'\ 16''{\cdot}8$. There can be no doubt about the approximate position from the sextant observations from *Altair* and *Antares*, as it is verified by comparison with the observed distances from θ *Scorpii* and ϵ *Sagittarii*. Since the year 1862 my library has been enriched by various Catalogues, but I could not find in any of them a star position corresponding to the above. My curiosity being aroused, I turned the 4½-inch Equatoreal on the spot in the evening of the 13th instant, and was surprised to

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find that the field contained stars of the 10th and 11th magnitude only. One star in particular attracted my attention: it was extremely faint, and could only be observed with great difficulty. By clamping the lower circle I measured the difference of Right Ascension between this star and θ *Scorpii* and σ *Aræ*, the difference of North Polar Distance being obtained from the readings of the declination circle. These comparisons gave the following position for the faint star:—R.A. = $17^h\ 30^m\ 21^s$; N.P.D. = $135°\ 23'$. The star was observed on the 13th, 14th, and 17th instant. Although there are several other faint stars within a few minutes of arc of this one, it is extremely probable that the star thus observed is identical with the star of the fifth magnitude by which the positions of *Brisbane* 5754 and 5799 were determined on the 4th, 5th, 6th, and 9th October 1862. The nebula seen in the same field of view with it is undoubtedly No. 3690 of Sir J. Herschel's Cape Catalogue for 1830. As the Variable is a remarkable one, I propose, when the Moon's absence allows the stars to be more readily seen, to record the positions of all the stars embraced in the same field of view.

Windsor, N. S. Wales,
November 21, 1877.

Improvements in a Solar Spectroscope, made by Mr. Grubb for Professor Young. By Wentworth Erck, Esq.

The usual practice in observing with the spectroscope, I believe, is to adjust the collimating and observing telescopes so as to render the incident and emergent pencils parallel for rays of mean refrangibility; and, on observing at either end of the spectrum, then to readjust the eye-piece of the observing telescope so as to render the emergent pencil parallel.

But this mode of procedure will not give the best definition, which requires that each pencil—that is to say, the pencil first incident on the first prism, as well as the pencil after dispersion—should be strictly parallel; and this adjustment of two variables can scarcely, so far as I am aware, be made while the spectroscope is in position.

Now, in the improved spectroscope alluded to, the collimating and observing telescopes are placed close together and parallel to each other, so that the observing telescope shall receive the rays after two internal total reflections.

In this position the eye-piece of the observing telescope and the collimating lens are coupled together by rack-work, so that both move together by turning one pinion; and thus, after the first adjustment, the two pencils are rendered strictly parallel for all parts of the spectrum.

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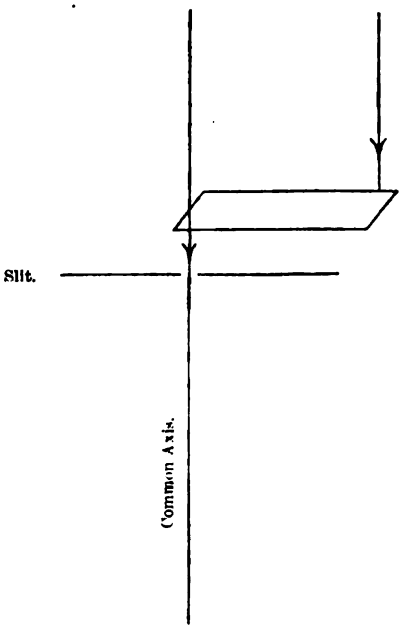
The power varies from 1 to 10 prisms of 60°.

The second improvement is in the mode of sweeping round the disk of the Sun.

It is not desirable to turn a heavy, complicated, and delicately adjusted instrument upside down, and into every conceivable position: there is great risk of the adjustments being injured by the flexure.

In the new spectroscope the axis of the collimating telescope is made to coincide accurately with the axis of the great telescope; the edge of the disk is then brought opposite the slit by the rotation of a small parallelopipedon of glass whose length is equal to the radius of the disk.



Thus the parallelopipedon alone rotates, while the spectroscope remains stationary.

Thirdly—The exact coincidence of the axes of the great telescope and the collimating telescope is obtained by mounting the spectroscope on a strong steel bar firmly attached to the great telescope, with a suitable adjustment between the bar and the spectroscope.

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On a Combined Position and Setting Circle to be used with Large Newtonian Reflectors. By Wentworth Erck, Esq.

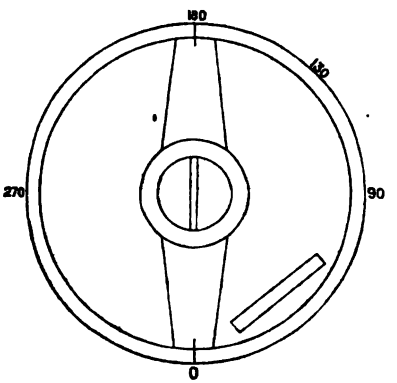
This instrument consists of an ordinary position-circle attached to the west side of the telescope, in such a manner that the diameter 90–270 shall be parallel to the axis.

The rotating eye-piece carries a pair of verniers, parallel to the lines in the eye-piece, in the usual way.

Attached to these verniers is a bubble in such a position that the diameter indicated by the bubble forms with the diameter indicated by the vernier an angle equal to the latitude.

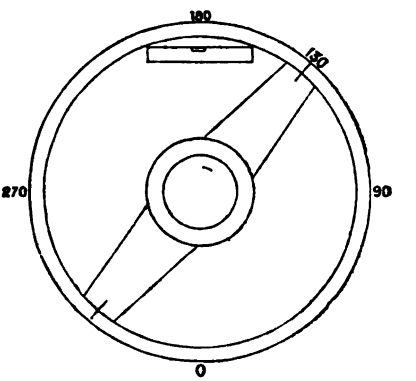
Under these circumstances it is evident that the vernier, when set by the lines in the eye-piece, will give the position of those lines; but when set by the bubble, the same verniers will give on the same circle the N.P.D. of the object to which the telescope is directed, provided the telescope be turned into the meridian.

Fig. 1.



Position Circle, reading 0—180.

Fig. 2.



Setting Circle, reading 120.

TELESCOPE DIRECTED TO AN OBJECT IN THE HORIZON.

When reading N.P.D. the circle rotates round a diameter fixed in position by the bubble; whereas, when reading positions a constant diameter rotates round a fixed circle.

It appears to me that in the case of large telescopes the setting circle is very much more accurate, more easily applied, and more economical than the declination circle, and in the case of very large instruments, where the speculum rests on the ground, it is the only mode of getting the N.P.D.

The above arrangement clearly brings the setting circle into the most convenient position for use.

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On the Variable Star U. Geminorum. By G. Knott, Esq.

In the month of December last the variable star *U. Geminorum* passed a maximum, which bad weather prevented my observing so completely as I could have wished. The date of maximum was probably about December 17th. The following are all the observations I was able to obtain :—

			h	m		Mag.
1877	December	12	10	15	G.M.T.	13·7
		13	8	45		Not > 13·7
		18	10	40		9·8
		23	12	50		10·9
		24	9	10		11·5
		26	9	5		13·5
		27	8	10		13·6
1878	January	7	10	15		13·7

Cuckfield, Jan. 11, 1878.

Note on the Mean Solar Parallax as derived from the Observations of the recent Transit of Venus. By Captain G. L. Tupman, R.M.A.

Shortly before the arrival of Mr. Stone's discussion of the British observations of the Transit of *Venus*, I had formed the opinion that the treatment of the *Ingress*, as exhibited in the Parliamentary Report, was very unsatisfactory.

I entirely agree with Mr. Stone as to the manner in which these observations should be combined, and I obtain for the *Ingress* a mean solar parallax of 8″·857, with mean error ±·040. As regards *Egress*, Mr. Stone has not yet seen the mass of Australian, Indian, and other observations now lodged at the Royal Observatory. From a discussion of the whole of these, I find the mean solar parallax 8″·792, with mean error ±·027.

From the general solution of the whole, limiting each observer to a single representative time for *Internal contact complete* and rejecting none, I obtain the mean solar parallax 8″·813, with mean error ±·033.

It appears to me that it is impossible to deduce from the observations a parallax much larger than this. Any other legitimate rendering of the Honolulu observations would very slightly reduce it.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. APRIL 12, 1878. No. 6.

LORD LINDSAY, M.P., President, in the Chair.

M. Camille Flammarion, Avenue de l'Observatoire 36, Paris; and

Alfred J. P. Shepherd, Esq., B.A., Queen's College, Oxford;

were balloted for and duly elected Fellows of the Society.

The Astronomer Royal communicated to the Council a letter relating to the erection of a statue to Le Verrier, addressed to him by M. Fizeau, President of the Academy of Sciences, and of a committee formed for promoting that object. The fund is to be raised by voluntary subscriptions. The committee includes already the President, the Vice-Presidents, and a large number of the Members of the Academy. Letters may be addressed to the Secretariat of the Institute of France, where a register of subscriptions has been opened.

The Total Solar Eclipse of 1878, *July* 29.

The following Notice to intending observers of the Total Solar Eclipse of 1878, July 29, has been issued by the Superintendent of the United States Naval Observatory:—

It having been suggested that various men of science in Europe desire to observe this eclipse, and that it might be possible, in some cases, to obtain for them reduced rates of fare on the railway lines of the United States, the case was presented to the Pennsylvania Railroad Company, and the following arrangements for a reduced rate of fare to and from the East to Denver, Colorado (which is near the Central line), have been

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offered by the Company. It will be noticed that these arrangements refer only to persons coming in a private capacity, and not to members of expeditions sent out by foreign governments.

[Extract.]

PHILADELPHIA, *January* 7, 1878.

SAMUEL M. FELTON, *Philadelphia.*

DEAR SIR,—Your letter of the third ultimo, addressed to THOMAS A. SCOTT, President, with enclosure from Professor EDWARD S. HOLDEN, U. S. Naval Observatory, Washington, requesting reduced rates from the East to Denver, Colorado, and return, for such scientific gentlemen from Europe as may wish to observe the total solar eclipse of 1878, July 29, has been referred to me for attention, and, in reply, I beg to state that the Pennsylvania Railroad Company is anxious to afford all possible facilities to persons coming in a private capacity for this purpose.

Upon the order of the Superintendent of the U. S. Naval Observatory, Washington, I will furnish transportation to Denver and return *viâ* Pittsburg, Indianapolis, St. Louis, and Kansas City, or *viâ* Pittsburg, Chicago, and Omaha, at the following rates for the round trip: From New York, $73.00; from Philadelphia, $71.00; from Baltimore or Washington, $69.50. . . . . .

L. P. FARMER,
General Passenger Agent.

The round trip ticket includes transportation (about 2,000 miles according to route) from New York to Denver and back again to point of starting; or in all 4,000 miles of travel.

These arrangements allow the journey to Denver to be made by one route and the return by another. The routes available are:—

No. 1. *Viâ* Pittsburg, Chicago, Omaha, Denver.
No. 2. *Viâ* Pittsburg, Chicago, Omaha, Denver.
No. 3. *Viâ* Pittsburg, Chicago, Omaha, Denver.

Different routes from Chicago to Omaha.

No. 4. *Viâ* Pittsburg, Chicago, Leavenworth, Denver.
No. 5. *Viâ* Pittsburg, Chicago, Atchison, Pueblo, Denver.
No. 6. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver.
No. 7. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Denver.

Different routes from St. Louis to Kansas City.

No. 8. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver.
No. 9. *Viâ* Pittsburg, Indianapolis, St. Louis, Kansas City, Pueblo, Denver.

Different routes from Kansas City to Pueblo.

Persons intending to make observations of this eclipse, and desiring to take advantage of the courtesy extended by the Pennsylvania Railroad Company to foreign men of science (by which the ordinary cost of transportation is reduced one-half), will please notify by letter the Superintendent of the U. S. Naval Observatory of their intention, and request that a letter certifying to their identity be forwarded to their Consuls at the port they may select for arrival in the United States. This

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letter must, upon delivery, be countersigned by the Consul of the applicant to prevent mistakes.

The Hon. Secretary of the Treasury has notified the collector of customs of the ports of Boston, Portland, New York, Philadelphia, and Baltimore of the probable arrival of European observers of this eclipse, who will be identified by exhibiting the above letter, so signed and countersigned, to the collectors named. Orders have been issued to them as follows :—

"Upon the arrival at your port of any of the gentlemen mentioned, you will extend all proper facilities for the speedy delivery to them of the professional instruments in question free of duty and charges."

On presenting the same letter, so countersigned, to the agent of the Pennsylvania Railroad Company in New York, Philadelphia, Baltimore, or Washington, a round-trip ticket to Denver and return will be issued to them by the Railroad Company at the rates before named.

From Denver railroad lines extend to points likely to be chosen for observing stations.

JOHN RODGERS.
Rear Admiral, U. S. Navy, Superintendent.

U. S. NAVAL OBSERVATORY,
Washington, February 1878.

The Luminous Spot on Mercury in Transit.

By B. G. Jenkins, Esq.

The attention of the scientific world will be directed on the 6th of May next to an astronomical occurrence of more than ordinary importance—a May transit of the planet *Mercury*, which has not occurred since the year 1845, although three November transits have taken place within the last thirty years.

Within the last 170 years there have been thirteen transits, upon which some observations on the luminous appendages of the planet have been made; but to come nearer to our own time, within the last forty-five years there have been in all six—on May 5, 1832; November 7, 1835; May 8, 1845; November 8, 1848; November 11, 1861; and November 4, 1868. It will be observed that four of these occurred in November, and only two in May. This is important, because the planet is in two opposite points of its orbit in these months—in November being only about five days from perihelion, and in May about ten days from aphelion, in which position it will be on the 6th of May next.

As we have not had a May transit since 1845, when it was only partly visible in this country, and the previous one in 1832 was unsatisfactorily observed, on account of the unfavourable weather, the coming transit will be of more than ordinary

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importance. It will be well in examining the transits to divide them into May transits and November transits, and, having compared them, to see what conclusions can be drawn in regard to the luminous appendages.

In the transit of 1799, which occurred in May, Schröter and Harding at Lilienthal, and Köhler at Dresden, saw a small luminous spot on the dark disk. The spot was not stationary, for Harding saw it change its position, and later in the day Schröter saw it sometimes on one part of the disk, sometimes on another. Other observers saw, not one, but two small spots of a greyish colour.

In the transit of May 1832 Professor Moll observed a spot, the periphery of which was not well defined, but was always situated in the same position, a little south of the centre of the planet, and preceding the centre.

Turning now to the November transits for further evidence of the luminous spot, we find that the transit of 1835 was not visible in this part of the world, and in the three previous ones of which we have some particulars, viz. those of 1736, 1789, and 1802, no mention is made of a luminous spot; but, as will be seen, it does not follow that it did not exist because it was not observed.

In November 1848 one spot was observed of a greyish colour, shading off on all sides from the centre.

In November 1861 Mr. J. W. Jeans saw a slight ashy light on the eastern limit or following the centre of the planet on its way to perihelion.

In November 1868 Mr. Huggins saw a point of light nearly in the centre which had no sensible diameter, but appeared as a luminous point. It was near the centre, a little south, and following the centre. It remained in the same position during the transit.

Mr. Browning, observing this transit, saw near the centre of the planet two bright grey spots close together, both a little south of the centre, one more conspicuous than the other.

So much for the luminous spot. Let us turn to the rings which have been observed round the planet during a transit.

In May 1707 the late Assistant of the then Astronomer Royal saw *Mercury* in transit encompassed by a thick haze or atmosphere.

In the transit of May 1753 a ring was observed round the planet; also in that of May 1786.

In May 1799 a dark or nebulous ring was observed, the tinge being of a violet hue and strongest near the planet. Schröter also saw the ring, the light of which he says was scarcely brighter than the surface of the Sun, but of another colour. Harding did not see it at first, but he did afterwards when he used a lighter shade.

In May 1832 the planet was seen at the Royal Observatory surrounded by a dusky tinge. In this transit also the ring has been described as of "a violet hue, the colour being strongest near the planet."

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This is the sum of our knowledge about the rings round the planet in the May transits, and from it the natural inference appears to be that when the planet is in transit near its furthest distance from the Sun, it is generally surrounded by a dark nebulous ring.

The first November transit in which we have mention of a ring is that of 1736. Professor Moll says Plantade saw the reverse of what he saw in May 1832; so that the ring must have been a bright one.

In 1789 and 1802 rings—probably bright ones—were observed. The transit of 1835 was not visible in Europe; that of 1848 was, however, and many observers saw a bright ring, although Mr. Breen says he did not see any.

In November 1868 Mr. Huggins saw the planet in transit surrounded by an aureola of light, a little brighter than that of the Sun's disk.

Here the weight of evidence appears to be in favour of a bright ring round the planet when near the Sun, which would probably be the case. It should be stated, however, that Prosperin calls the ring of May 1786 a luminous one, and Ljungberg that of November 1802 a dark one; but it must be remembered that the ring of the transit in 1799, which there can be no doubt was dark, nebulous, and of a violet hue, is described by Schröter as scarcely brighter than the surface of the Sun, and of another colour.

Collecting these results and comparing them, it will, I believe, be found:

1st. That in the May transits, when *Mercury* is near its aphelion, the luminous spot is in advance of the planet, preceding the centre; in the November transits, when *Mercury* is near its perihelion, the luminous spot follows the planet.

2nd. The luminous spot has never been seen at the centre, but always south of it, and therefore cannot be due to diffraction, as Professor Powell suggests.

3rd. Sometimes in the same transit two spots have been seen close together, where shortly before only one was observed.

4th. In the May transits the rings round the planet were dark or nebulous and of a violet tinge; in the November transits they were bright.

5th. If we take the two transits which have received the most careful observation, the May transit in 1832, observed by Professor Moll, and the November transit in 1868, observed by Dr. Huggins, we find the contrast very great and very typical: in the one case a diffused spot preceding the centre, with dark ring surrounding the planet, in the other a sharply defined spot following the centre, with bright ring surrounding the planet.

6th. From all which I conclude that on May 6 next, when *Mercury* will be at almost its furthest distance from the Sun, the luminous spot will not be a point of light having no sensible diameter, but rather diffused, with a periphery not well defined,

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gradually sinking from a greyish white to the dark colour of the disk, situated a little in advance of the centre and to the south of it, and that the planet will be surrounded by a dark nebulous ring, not a bright one.

4 *Buccleuch Road, Dulwich,*
March 25.

Early Transits of Mercury. By the Rev. S. J. Johnson.

On occasion of the transit of *Mercury* in 1861 I found the planet distinctly perceptible on the Sun with an old ship glass carrying a power of 16. Between the introduction of the telescope in the year 1610 and Gassendi's observation in 1631 three transits of *Mercury* took place, each of which was more or less visible to Europe. These were

1615	May 3	Ingress about $6\frac{1}{2}^h$ morn.	Egress about $1\frac{1}{2}^h$ aftern.
1618	Nov. 4	„ „ $11\frac{1}{2}$ morn.	„
1628	May 5	„ „ $2\frac{1}{4}$ aftern.	„

As many telescopes at this period were probably twice as powerful as the glass I have referred to, is it not possible there may be diagrams of Sun-spots extant showing the little round speck of *Mercury*? Galileo's *Macchie Solari* gives several solar spot diagrams for 1613. Scheiner's *Rosa Ursina* contains several for the year 1625, but there are no doubt other works giving the like.

A year before Horrox obtained a successful observation of a transit of *Venus* he looked unsuccessfully for one of *Mercury*. As this is not referred to in any popular modern work, his words may be given:—

"1638, Octob. 20.—Paulo post ortum solis vidi, per telescopium maculas duas in sole, easdem vidi meridie et diebus sequentibus. At *Mercurium* non vidi. Ex calculo Lausbergii fuisset *Mercurius* in centro Solis horâ 5 manè.

Octob. 21.—Manè. Nullum vidi *Mercurium* in sole, ergo *Mercurius* vel ob latitudinem non ivit sub Solem, vel fuit conjunctio nocte præcedente: flavit enim ventus insolitâ violentiâ. Calculus Rudolphinus facit conjunctionem horâ 11·47′ quâ ego non observavi. Observatio autem Gassendi A.C. 1631 Octob. 28 probat conjunctionem fuisse citiùs per horas 4·43. Ergo vera conjunctio fuisset horâ 7·4′ paulo ante ortum Solis: at statim post ortum Solis nihil visum. Ergo latitudo *Mercurii* major erat semidiametro Solis. Quod etiam probat Gassendi observatio." (*Catalogus observationum Jeremiæ Horroccii*, 1672).

Horrox was at Toxteth at the time, before he went to Hoole. His failure does not appear to have been from insufficient telescopic

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power. On making an approximate calculation of the places of *Mercury* and the Sun by the same method as for the future transits of the planet (*Monthly Notices*, May 1877) I found *Mercury* slightly escaped a passage across the Sun's disk on this occasion.

Upton Helions Rectory, Crediton,
April 9, 1878.

A Comparison of the Observations of Contact of Venus with the Sun's limb in the Transit of 1874, *December* 8, *made at the Royal Observatory, Cape of Good Hope, with the corresponding observations made at the stations in the Parliamentary Report, and a discussion of the results.* By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Royal Observatory, Cape of Good Hope.

The Cape observations of the times of contact of *Venus* with the Sun's limb at Egress on 1874, December 8, were immediately after the transit forwarded to Greenwich to be worked up with observations of contact made at the other stations. The Cape observations were not, however, included in the Parliamentary Report, and I should not have felt myself at liberty to anticipate the official publication of the results were it not that some values of the solar parallax, supposed to have been derived from their use, have been published in the *Observatory* Magazine for 1878, February. The value of the solar parallax which is given in that magazine, as the result of a comparison between the Cape observations of the phase ζ, or what I call the "real contact," and the corresponding observations at six stations where the contact was retarded at Egress, is $8''{\cdot}495$. I feel certain that no such value of the solar parallax can be fairly derived from our observations, and I have taken some trouble to test this matter by direct computation and comparison of the results. At the time of the transit there were only two instruments at the Cape fit for the delicate work of observing contacts for a determination of a value of the solar parallax. I observed the contacts with the 7-inch telescope by Merz and employed a power of about 200; Mr. Finlay, my first assistant, used a good 4-inch telescope by Dollond and employed a power of about 180. The contact was also observed by Mr. G. Maclear with a small theodolite of which the object glass had an aperture of 1·65 inches. He employed a power of only 23. With such optical means it was of course impossible for Mr. Maclear to attempt to discriminate between changes of the Sun's limb which are presented near the point of contact, and his observation is not available for comparison with contacts made with larger instruments and with greater optical power. But I thought that an observation made by a careful

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observer with small optical power might be useful in discussions respecting the nature of the contacts observed.

The following are the contacts observed:—

1874, Dec. 8.

Observer, E. J. Stone.

	Cape Mean Times. h	m	s	
(1)	19	6	25	Slight connection, but not firmly established; good.
(2)	19	7	12	Continuous now; a fair breadth.
(3)	19	8	5	Apparent contact.

At the apparent contact a considerable portion of the Sun's limb was cut out, or the ligament was about one-third of the diameter of *Venus*.

Observer, W. H. Finlay.

	h	m	s	
(2)	19	7	10·7	Permanent connection seen.
(3)	19	8	18·3	Apparent contact; considered rather late.

The time, $19^h\ 8^m\ 18^s{\cdot}3$, has been increased 30 seconds for error of count by Mr. Black. Mr. Black noticed at the time of making the entry that he was then counting from the wrong end of the seconds hand. A note of this and of the effect of the error was made at the time, and before any comparison had been made with other observations.

Observer, G. W. H. Maclear.

h	m	s	
19	7	48	First permanent contact.

There can be no doubt about the selection of the contacts which, at the Cape, correspond to the phase ζ. If we take the mean of my time, $19^h\ 7^m\ 12^s$, and Mr. Finlay's, $19^h\ 7^m\ 10^s{\cdot}7$, we have $19^h\ 7^m\ 11^s{\cdot}35$ Cape mean time for the observed phase.

The following table is extracted from the *Observatory*, 1878, February.

Results from Egress—Phase ζ.

Stations.	Acceleration. s	Parallax. "	No. obs.	Weight.
Sydney (N.S.W.)	388	8·768	5, 11	77
Goulburn (N.S.W.)	388	8·891	2, 11	54
Woodford (N.S.W.)	382	8·801	1, 11	36
Victoria	406	8·900	3, 11	64
Adelaide	339	8·778	3, 11	57
Cape Observatory	187	8·495	2, 11	30
Port Elizabeth (C.G.H.)	165	8·385	1, 11	21

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Stations.	Retardation. s	Parallax. "	No. Obs.	Weight.
Mokattam and Suez	594	8·774	4, 17	96
Luxor (Egypt)	562	8·780	3, 17	78
Abbasseyah (Cairo)	594	8·780	1, 17	45
Roorkee (India)	544	8·804	1, 17	45
Madras (India)	426	8·918	1, 17	31
Mussoorie (India)	563	8·597	1, 17	45

I have not all the materials which have been used in the determination of the value 8″·495 placed opposite the Cape Observatory; but, as the Cape observations are only compared with 11 observations of Egress retarded, and as the mean value assigned to the last 4ˢ scarcely differs from the mean value found from the first 7, if the value 8″·495 be correct some such value must be found from a comparison with the 7 contacts observed at Mokattam and Suez, and Thebes which are printed in the Parliamentary Report.

I have therefore computed the tabular distances between the centres for my own station and for the stations Mokattam and Suez, and Thebes. The value of the solar parallax adopted is 8″·90. The following are my results :—

	′	″	
Mokattam and Suez	15	41·679 − 2·257 $\delta\pi$ = C	4 obs.
Thebes	15	41·783 − 2·134 $\delta\pi$ = C	3 obs.
Cape	15	41·879 + 0·616 $\delta\pi$ = C	2 obs.

The agreement between these results is so close that really no sensible correction to the assumed value of the solar parallax would appear necessary. However, if we combine the Cape and Suez we find

$$\delta\pi = -\frac{0\cdot200}{2\cdot873} = -0\cdot069, \quad \text{or} \quad \pi = 8''\cdot831;$$

and if we combine the Cape and Thebes, we find

$$\delta\pi = -\frac{0\cdot096}{2\cdot750} = -0\cdot035, \quad \text{or} \quad \pi = 8''\cdot865;$$

whence, in the mean,

$$\pi = 8''\cdot846 \pm 0\cdot22\ \epsilon.$$

The value of the solar parallax thus deduced is well within the probable error, if the true value of the solar parallax be as large as 8″·90.

I am at a loss to understand how the value 8″·495 can have been obtained from the Cape observations, unless the time of Mr. Maclear's contact, $19^h\ 17^m\ 48^s$, has been virtually adopted

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as the time of contact, and a second contact agreeing with this has been obtained by taking Mr. Finlay's apparent contact for a real contact, and neglecting to apply the 30 seconds required for error in count, or by erroneously correcting his time for phase ζ by 30 seconds. But in these cases all my observations must have been deliberately rejected, and Mr. Finlay's unduly dealt with. I can hardly believe that such steps can have been taken in an important investigation, but I can see no other explanation which will account for the value of the solar parallax deduced from the Cape observations.

I will now compare all the three kinds of contact which I observed at the Cape with the corresponding observations given in the Parliamentary Report. The adopted value of the mean solar parallax is 8″·95 which is the value of the Parliamentary Report.

First Phase, or δ.

We have five observations available, four from the Parliamentary Report and my observation at the Cape. The corresponding distances between the centres are

		′	″
	Mr. Hunter, Suez	15	40·08
	Mrs. Campbell, Thebes	15	39·31
(1)	Lieut. Hoggan, Rodriguez	15	40·30
	Lieut. Goodridge, Kerguelen	15	40·61
	Mr. Stone, Cape	15	40·38
	Mean	15	40·14

The Cape observation of this early phase therefore agrees within o″·24 with the mean of the five observations.

Second phase or ζ.

We have for the distances between the centres

		′	″	
	From 4 obs. at Suez	15	41·46	
	„ 4 „ Thebes	15	41·37	
(2)	„ 5 „ Rodriguez	15	41·43	
	„ 2 „ Cape	15	41·79	Stone & Finlay
	„ 3 „ Kerguelen	15	41·28	
	The mean, with weights, is	15	41·44	

The Cape result therefore only differs o″·35 from the mean. I have taken for Kerguelen the mean of the three observations by Perry, Sidgreaves, and Goodridge.

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Third phase or η.

We have the following observations of apparent contact:

	2 obs. at Suez	15	44·54	
(3)	3 „ Rodriguez	15	43·31	
	2 „ Cape	15	43·61	Stone & Finlay
	The mean is	15	43·75	

and the Cape observation differs 0″·14 from the mean.

I have given these comparisons and called attention to the close agreement between the Cape results and the mean results for all the stations because, when my observations were first sent to England, I was told that I had allowed my theoretical views to influence my observations. The comparisons here given afford my answer to the charges thus brought against me, and are a proof of the correctness of my theoretical views. An inspection of the results for the phases (1), (2), and (3) will show that no mistake need be made in referring an observation at least to its proper class. But if an observation of phase (1) at one station be combined with an observation of phase (3) at another station, the result must be an erroneous value of the solar parallax to the amount of about 3″ divided by the sum of the parallax factors for the stations under consideration; or, if an observation of what I call real contact, or phase (2), be combined with an apparent contact, or phase (3), the result must be an erroneous value of the solar parallax to about the amount of 2″ divided by the sum of the factors. But if observations of phase (2) be alone combined, the discordance should be confined within comparatively speaking small limits. The method is a strong one, but requires, of course like all other methods, to be correctly applied. The views here expressed are those upon which I acted in my rediscussion of the transit of 1769, and without their substantial adoption no discussion of the observations made in 1874 is possible.

If we attempt to obtain a value of the solar parallax from the few observations of the phases δ and η collected in the Admiralty Report with the addition of only the Cape observations, it must of course be expected that the results should run wild. The probable errors of such results must be considerable, and the theory of probable errors itself is hardly applicable in a discussion of such small groups of observations. I shall, however, exhibit the results partly as a matter of curiosity and partly to show how, by mutual elimination of error, even a few observations rapidly approximate to the true result. The five observations of phase δ lead to the value

$$8''{\cdot}69 \pm 0{\cdot}322\ \epsilon, \quad \text{or} \quad 8''{\cdot}69 \pm 0''{\cdot}161,$$

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and the seven apparent contacts to the value

$$9''{\cdot}31 \pm 0{\cdot}331\ \epsilon, \quad \text{or} \quad 9''{\cdot}31 \pm 0''{\cdot}166.$$

If we combine these results, we have

$$\pi = 8''{\cdot}996 \pm 0''{\cdot}116,$$

a result which, by its probable error, touches the value 8″·88.

If we combine the Cape observations of phase ζ with the other observations of the same phase collected in the Parliamentary Report, and use, as in my paper of January, the mean of all the contacts observed at Kerguelen for the phase ζ, we have the following equations:

No. Obs.		Residuals. (Obsd.—Compd.)
4	$x + 0{\cdot}202\, y = 0{\cdot}455$	+0·035
4	$x + 0{\cdot}191\, y = 0{\cdot}368$	−0·053
5	$x + 0{\cdot}027\, y = 0{\cdot}426$	−0·017
2	$x - 0{\cdot}057\, y = 0{\cdot}793$	+0·340
3	$x - 0{\cdot}100\, y = 0{\cdot}283$	−0·176

These equations lead to

$$18\, x + 1{\cdot}293\ y = 0{\cdot}7857$$
$$1{\cdot}293\, x + 0{\cdot}3428\, y = 0{\cdot}53096,$$

from which

$$x = 0''{\cdot}446, \qquad y = -0{\cdot}13,$$

or, since $\pi = 8{\cdot}95 \left(1 + \frac{y}{100}\right)$,

we have

$$\pi = 8''{\cdot}938 \pm 0{\cdot}175 \times \epsilon,$$

a result which is, I believe, within its probable error of about 0″·053.

The value which I obtained from the Ingress observations was $8''{\cdot}860 \pm 0{\cdot}136\ \epsilon$.

The result of the introduction of the Cape observations is therefore slightly to diminish the large value derived from the Egress observations when the Kerguelen observations, which were picked up through cloud, were alone available for accelerated Egress. Of course if the Kerguelen observations are not good (I mean good in the sense of being strictly comparable with the observations made at the other stations), then the observations of Egress made in Australia may modify this result; but the value of the solar parallax must, I believe, lie between 8″·84 and 8″·92. The value of the solar parallax at present entitled to the greatest weight appears to me to be

$$8''{\cdot}88 \pm 0''{\cdot}02.$$

1878, *March* 12.

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Addendum.

I received yesterday morning copies of the Port Elizabeth observations, the originals of which were forwarded to England in April 1875. There are three times of contact given:

	h	m	s	
(i)	7	25	13	[The correction to reduce these to Cape Mean Time is about $-18^m\ 6^s$.]
(ii)	7	25	23	
(iii)	7	25	38·5	

"When observation (i) was recorded the contact seemed so nearly perfect that only a thin dark line along the edge of the Sun proved the contact hardly consummated.

"When observation (ii) was recorded the appearance of the limb was as though blended in one sharp defining line: hence set down as the moment of perfect contact.

"When observation (iii) was recorded the contact at that moment may be described simply as fully perceptible or certain but with no observable loss of its circular form in any appreciable degree."

The above was the description of contact (i) in the original paper. If that description had been correct these observations would appear to have been observations of apparent contacts, but then a comparison with the Cape observations appeared to show that they must be a minute wrong. I wrote therefore in 1875, asking how long before (i) the dark line was first seen, and whether there was any possibility of a mistake having been made in the minutes. The answer was that the *dark* line was a mistake; the line should have been described as a *bright* line, and that the minutes were thought to be correct. The change of the word *dark* to *bright* of course changed the interpretation to be put on these observations, which must be attempts at real contact, and in this case the minutes are correct. If we take the mean times given for the contacts (ii) and (iii), as we have done at Rodriguez and Kerguelen, the equation of condition for Port Elizabeth is

$$15'\ 41''{\cdot}64 + 0{\cdot}564\ \delta\pi = C,$$

and this, combined with those for Thebes and Mokattam and Suez, shows that the value $8''{\cdot}93$ satisfies the Port Elizabeth observation. I can only account for the value $8''{\cdot}385$ assigned to the Port Elizabeth observation in the *Observatory* magazine by supposing that the minutes have been erroneously altered. The only ground for supposing the minutes wrong was the idea at first that the observations were apparent contacts, and this idea is disproved by the bright line seen to separate the limbs only 15' before the contact. This will be clearly apparent on comparison of the phases δ, ζ, and η given at the different stations.

1878, *March* 12.

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Photography at the Least Refrangible End of the Solar Spectrum.
By Capt. Abney, R.E., F.R.S.

Nearly two years ago I had the honour of reading a preliminary note on photographing the least refrangible end of the spectrum, and it seems that the time has come when I ought to redeem the promise implied by a "preliminary note" and enter further into the subject. Last year, owing to a change in residence, I was unable to pursue the subject with any degree of activity, but during this last winter and the present spring I have made fair progress in my researches, the results of which I lay before the meeting. These results are principally photographs themselves, and the first to which I shall call attention is one taken through three prisms of dense flint glass, each of which had a vertical angle of 62°. They were placed at the angle of minimum deviation of B and kept so. The focal lengths of the collimator and camera were 18 inches and 2 feet respectively, and a condensing lens of 6 feet focus was employed to collect the light, the middle of the collimating lens alone being filled with solar rays. In front of the slit was placed a plate of orange glass, in order to cut off the suffused blue rays, which experience had previously taught me were inimical to the production of good negatives, owing to the light dispersed in the prisms themselves. It will be noticed to what an enormous distance below A the impressions of the bands in the ultra red are to be found. If the wave lengths be used as abscissæ and the measured distances of the known lines be used as ordinates, it will be found, if the waves be completed by hand, that a wave length of not less than 10,400 tenth metres is impressed. Roughly speaking, A is 7,600 tenth metres and D 5,900 tenth metres, by which it will be seen that I was within the mark when I announced that I had obtained photographs as much below A as D was above it. Now this negative, though interesting as a feat in photography, has no practical scientific value, as the ultra red is so tremendously compressed that the absolute wave lengths could not be obtained from it.

About the time I read my last paper, Captain Tupman kindly lent me a speculum metal grating, by Rutherford, having about 8,600 lines to the inch; but it was only lately, after removing its glass covering, that I was fully able to appreciate its value. In all gratings the red, or rather the ultra red, of the first order is overlapped by the ultra violet and violet of the second order, and the higher the order the more overlap there is. To remedy this, which was a defect for the purpose for which I required it, I placed before the slit of the collimator red glass, which completely cut off the yellow and only allowed a little of the green to pass. The prisms were replaced by the grating, and a photograph of the second order of the spectrum was taken by it. Theoretically speaking, the time necessary for taking a photograph of the first order is $\frac{1}{8}$ that for taking one of the second, and $\frac{1}{27}$ for that of the third. Practically, this is not quite true, for reasons which it is

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unnecessary to enter into here. This induced me to think that if I could get a grating with double the number of lines to the inch, its first order should give me the same dispersion as the second order of the grating I was using, and at the same time the exposure ought to be more than halved. Mr. Lockyer kindly lent me such a grating, and the third photograph was taken by it. You will see that it gives the lines from C to A almost perfectly. A positive copy of this photograph I sent to Professor Piazzi Smyth, as he drew the map of this region of the spectrum which he published in the last volume of the *Edinburgh Astronomical Observations*; and I cannot do better than quote his words regarding the accuracy of the photograph :—"As the size of the glasses approximated closely to my standard camera size, 2·25 inches × 4·25 inches, I was able at once to view them with any magnifying power in a compound microscope long since arranged for such things, and the effect was astounding. I almost thought I was back again in Lisbon, viewing the Sun's spectrum itself as I used to see it."

This acknowledgment of the value of the photograph was particularly gratifying, as it came from an astronomer who had paid particular attention to this particular part of the spectrum.

The next plate shows the region of the ultra red as taken with the same grating. The most conspicuous group has a wave length of about 8,400 tenth metres and the extreme line visible has a wave length of about 9,200 tenth metres. At this point the closer ruled grating seemed to fail me, and I could not get beyond this point. It then struck me that the glass on which the lines were ruled might absorb the rays beyond (for I must explain that this grating was ruled on glass and silvered at the back) or that the red glass might absorb them. I then reverted to the first grating and adopted a different method of proceeding.

This was Fraunhofer's method, to which I was practically new—though I believe I have been credited (though inaccurately) with using it for other experiments—and after various attempts the following is an outline of the arrangement adopted. The slit was placed horizontally, a prism of 60° occupied a position at the end of the collimator next the lens, the reflection grating then received the rays and reflected them into the camera lens, the camera being tilted at an angle to make them fall on the sensitive plate. To show what sort of effect can be got I exhibit a photograph in which from the first to the fourth order of spectra were impressed on the same plate in a small camera. When this method was applied to the larger apparatus I got further into the ultra red than I had done before, and such a plate you see before you; the wave length of the last line of my group of lines visible on the plate is about 10,300 tenth metres. From indications on other plates I am inclined to think that we may get as far as 12,000 tenth metres, which it will be admitted is a tolerable distance to travel along the

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invisible spectrum. In these ultra regions the lines are faint, but perfectly measurable under a moderate magnifying power, and I have therefore proposed to myself to make a map of the ultra red. For the purpose I propose to use the overlapping of the higher order of the spectrum over that used. By cutting off the red in one case and using half the length of the slit for one exposure, and then by cutting off the blue and using the remaining half of the slit for the other, we shall have one spectrum over the other. The wave lengths of the most refrangible rays are known, and since the dispersion of the higher order is double the one below it, the wave lengths of the latter can be accurately ascertained. When once a scale is obtained the greatest difficulty will have vanished.

Now as to the process. My object has been to weight the molecules of silver bromide that they may absorb the red rays. With ordinary silver bromide the film allows these very rays to pass through whilst a blue absorption takes place. In other words, my endeavour has been to find a heavier molecule of a sensitive salt, which shall answer to the swing of the waves of the red and ultra red rays. This I first accomplished, as I stated at the time, by adding resins to the silver salt and forming what I may call a bromo-resinate of silver. But I am happy to say that I have secured the same end by, I believe, doubling the molecule of the silver bromide. Now this doubling the molecule is a matter of manipulation more than of chemical knowledge, and I might describe the process in detail, as I have already done in papers I have published, and yet the double molecule would not be obtained unless careful manipulation was attended to—manipulation easy to follow when seen, but difficult to follow from any description. I should therefore prefer to teach practically anyone who is acquainted with silver bromide emulsion making, rather than allow him to be misled by what must be imperfect directions.

I exhibit two films, both prepared with sensitive emulsion, which is composed of exactly similar ingredients, viz. pure silver bromide. You will at once note the difference in colour of the light transmitted. The one which is sensitive to the red and ultra red is of a blue tint; the other is orange. You will see that the blue tint would appear to be due to a physical arrangement of the molecules, for if a part of the film be rubbed you will see that it changes to a ruddy tint, passing through an emerald green stage. We have it so on the screen. Whether it be right or wrong in regard to other matter, I am convinced that in silver bromide we have the possible existence of two sizes of molecules. In the blue film we have the presence of both; when only the larger size is present we shall have a compound which is very much more sensitive to the lower end of the spectrum than it is to the upper. Allow me to say that these views are not original except in so far as they are applied to this subject. Whether they are correct or not, they have formed a good working hypothesis which has led me to the results obtained.

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Before closing I must refer to the comparative lengths of exposure required for these photographs. My impression is that at A the exposure required is about 25 times that required for G, in a fairly bright sun at midday; for the ultra red, as far as I have gone, I should say about 35 times. These are only approximations, but still will enable you to form some idea of the sensitiveness. I show you a photograph taken about 4 P.M. on March 18. The exposure was about 50 seconds. You will see that the red end is as strong as the blue, with the yellow much lacking in density. In other words, the yellow rays are nearly inactive.

The photograph which I showed you of the four orders of the spectrum was taken on April 3, at 2.30. It had an exposure of one minute and a half. The photograph in which the furthest band of lines was seen had an exposure of 12 minutes on the 8th, at 2.30. The slit was in this case closed as nearly as possible.

In conclusion, I have to remark that in a short time I hope to reduce the exposures considerably. In the course of some investigations, the results of which have just been communicated to the Royal Society, I found that the red rays could oxidise a photographic image as well as form it, and that in an oxidised state it was unable to be developed. If the tendency of the sensitive compound to become oxidised exceeded its tendency to become reduced, no image could be developed. By exposing *in vacuo* or in a nitrogen atmosphere I hope to eliminate altogether this oxidising effect, and so get firmer images.

I trust that this paper may be considered as the fulfilment of the promise implied in my preliminary note.

On the Duration of Meteor Showers. By R. P. Greg, Esq.

In the No. of the *Monthly Notices* of the Society for January 1878, Mr. Denning has a paper on "Suspected Secondary or Repetitive Outbursts from Radiant-points; and on the long Duration of Meteor Showers"; and Captain Tupman has appended some valuable remarks on Mr. Denning's paper.

1. In the case of a *second* outburst of the same supposed meteoric radiant, some two, three, or even four months after the first (supposing it to be a true case of double intersection of the same meteor orbit, say near its perihelion), then I imagine it could easily be shown that the apparent positions of the radiant, on each occasion, must necessarily lie at least in different quarters of the heavens; and it would be an almost impossible matter to decide whether the two radiants belonged to one and the same shower and orbit; unless indeed the individual meteors were wonderfully characteristic and similar in appearance.

2. With respect to the *duration* of meteor showers, though I

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think Mr. Denning may rather exaggerate that element, my own experience would certainly favour the idea of a longer average duration for meteor showers having a very fairly decided fixed radiant than one could have supposed likely. In *not* a few cases the duration would even seem to endure for two or three months without any special intermission. But leaving out such extreme cases, as caused probably by two distinct showers overlapping each other as it were, let us consider the *average* duration of meteor showers, so far as can be judged from some of the principal Catalogues. In the late Professor Heis' latest Catalogue (1877) there are specified no less than 120 different meteor showers with their radiant positions for the six months July to December. The *average* duration I find for each is 20½ days and the number of meteors recorded about 6,000, without any undue allowance for *Perseids* and *Leonids*, and there are 14 showers lasting over 34 days. The observations extend over a period of 25 years.

The average duration of 105 showers, 2,300 meteors, as deduced from Mr. Denning's reductions of the Italian observations, 1872, July–December, is 24 days. Mr. Denning's own observations, reduced from 2,170 meteors in 1876–7, give an average of 22 days.

In Dr. Schmidt's Catalogue there are 45 meteor showers with duration of about 30 days or more. In my own Catalogue, reduced from 2,000 meteors seen in England 1849–1867, the average duration for 40 showers for the whole year is 33 days (omitting 12 showers over 54 days, some of which are doubtless not really one shower).

I think we may therefore take it for granted, until at least proved to be incorrect, that the *average duration* of a meteor shower having a pretty constantly fixed radiant area, say of from 3°–8° in diameter, is not less than *three weeks*. Now, as some of these meteor showers only endure for *one* or *two* days, it is not unreasonable to assume for a *maximum* a duration of even *six weeks*; that is to say, meteor showers, so far as ascertained, endure from one to at least 40 days, giving in most cases a pretty fixed radiant in the heavens. There are not an inconsiderable number of cases in which the duration would appear to be even as great as 50 or 60 days, but this would appear to be so surprising that further proof and long continued nightly watching and recording is requisite to confirm it. The *Perseids* belong to a shower having a strong *maximum*, as is well known, about August 10, but it is perhaps not equally well known that this shower feebly commences about July 24 and continues until about August 17, ending much more suddenly than it commences.

The *Leonids* last for a few days with a strong maximum of only a few hours; the *Andromedids* for not more than half-a-day.

When, as we have shown, it may appear highly probable that the *average* duration of a meteor shower (of which at least 200 are now known whose orbits are intersected by the Earth) is

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about *three weeks*, it must stand to reason that a considerable number may be expected to show a duration of at least five or six weeks, if not very occasionally more.

Captain Tupman has clearly pointed out the special conditions required to produce a nearly fixed meteor radiant for several weeks, viz. "The meteor orbit must nearly coincide with the plane of the ecliptic, the perihelion distance of the central position be a little less than unity, and the motion direct. The position would be 90° before the Sun at the middle time."

I am inclined, however, to think that another reason, which may assist in explaining the apparent difficulty of a rather frequent *long* duration with fixity of radiant, may be found in the supposition that the zone or ring of meteors belonging to a majority of showers may be a good deal wider or more diffuse than hitherto supposed, possessing a width in fact of several millions of miles, and this at the same time not improbably coincident with a considerable degree of parallelism in the direction of the orbits of the Earth and meteors at the time.

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*On the Solution by Trial of Lambert's Theorem, in Olbers' Method for the Computation of Parabolic Orbits.** By R. H. M. Bosanquet, Fellow of St. John's College, Oxford.

In all the different forms of the method of Olbers for the computation of parabolic orbits there is a certain step, depending in principle on the finding by trial of values of the heliocentric radii vectores of the comet which satisfy the condition given by the application of Lambert's Theorem to the observed times.

In the Appendix to Olbers' treatise, at pp. 68–71 of Encke's edition (1864), a method is given by which, from any assumption as to the value of the chord joining the first and third places of the comet, or of the sum of the corresponding geocentric radii vectores, more accurate values can be deduced by Lambert's theorem. This discussion contains the germs of the principal subsequent improvements of the method; and the form given to Lambert's theorem in the Note (Encke's *Olbers*, p. 70) is that on which the present discussion will be based.

In the Gaussian modification of Olbers' method, a plan is commonly adopted which is in principle the same as that recommended by Olbers in the place cited. It consists in assuming a value of u, for which Encke has given the formula

$$\left\{\left(\frac{t''-t}{4t}\right)^2 - A^2\right\}^{\frac{1}{2}},$$

* A paper on this subject was written by the author some years ago, and appeared in the *Astronomische Nachrichten* in 1872. On reading it over the author thinks that the method may still be worth the attention of the Society. The investigation is substantially unaltered, but the discussion of the example has been revised.

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determining r and r'' (heliocentric radii vectores) on this hypothesis; thence k^2 (square of chord) by the auxiliary table of Encke, and then u again from the equation

$$k^2 = \mathfrak{u}^2 + A^2.$$

The value of u so obtained is generally better than that originally assumed, just as in the original rules of Olbers the value of k^2 resulting was better than that originally assumed.

So far as I know, however, the conditions of this improvement have not been directly investigated; and it has seemed to me worth while to examine under what conditions and to what extent this improvement takes place. I have been thus led to the following simple method, by which it would appear that, after calculation of two hypotheses as to the value of u and of a certain formula, a value of u may be deduced nearly as accurate as five-place calculations will admit.

Let the value of u first assumed be called u_0, and let that obtained after calculation of r, r'', and k^2 be u_1. Let the true value of u be called u, and let

$$u_0 = u + \delta u_0, \qquad u_1 = u + \delta u_1.$$

If we then seek a relation between δu_0 and δu_1, then by means of this relation and the equation

$$u_0 - u_1 = \delta u_0 - \delta u_1,$$

the values δu_0, δu_1, can be determined, and a value of u deduced closely approximating to the true value required.

In fact, if we combine a result of the form

$$\delta u_1 = -F\,\delta u_0,$$

with

$$u_0 - u_1 = \delta u_0 - \delta u_1,$$

we get

$$\delta u_0 - \delta u_1 = -\left(\frac{1}{F} + 1\right)\delta u_1 = u_0 - u_1,$$

and therefore

$$\delta u_1 = -(u_0 - u_1)\frac{F}{F+1};$$

whence

$$u = u_1 - \delta u_1$$

$$= u_1 + (u_0 - u_1)\frac{F}{F+1},$$

which is the expression for the improved value of u.

To deduce an expression for F:—

Putting $u + \delta u_0$ for u in the Gaussian equation

$$r^2 = \left(\frac{u+c}{h}\right)^2 + B^2,$$

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we have for the corresponding error in r

$$\delta r = \delta u_0 \frac{u+c}{rb^2},$$

neglecting the squares of the errors; similarly

$$\delta r'' = \delta u_0 \frac{u+c''}{r''b''^2};$$

and if we write $r+r'' = S$,

$$\delta S = \delta u_0 \left(\frac{u+c}{rb^2} + \frac{u+c''}{r''b''^2}\right). \qquad (I)$$

Now if in Lambert's theorem we put

$$\vartheta = \kappa\tau, \ (\log \kappa = 8{\cdot}5366114, \text{ Encke's } \textit{Olbers}, \text{ p. } 251),$$

where τ is the interval of time between the first and last observations, we can expand k^2, the square of the chord joining the first and last positions of the comet, by means of Lambert's theorem, in terms of ϑ and S. Thus:—

$$k^2 = \frac{\vartheta^2}{S} + \frac{\vartheta^4}{12S^4} + \frac{\vartheta^6}{36S^7} + \ldots.$$

Encke's *Olbers*, p. 70, Note.

If we write $\eta = \dfrac{\vartheta}{S^{\frac{3}{2}}}$, the series becomes

$$k^2 = \frac{\vartheta^2}{S}\left\{1 + \frac{\eta^2}{12} + \frac{\eta^4}{36} + \ldots\right\},$$

where the series within the brackets is μ^2; $\log \mu$ being the quantity which is given with η as argument in Encke's table for the solution of Lambert's theorem.*

In the series for k^2 write $k^2+\delta(k^2)$ for k^2, and $S+dS$ for S; then

$$\delta(k^2) = -\frac{\vartheta^2}{S^2}\left(1 + \frac{\vartheta^2}{S^3}\right)\delta S$$

$$= -\eta^2 S\, dS, \qquad (II)$$

omitting the terms of the expansion after the first, which may be done for approximate purposes.

Again the equation $k^2 = u_1^2 + A^2$ gives

$$\delta(k^2) = 2u\delta u_1. \qquad (III)$$

* The expression $k = \dfrac{\vartheta}{S^{\frac{3}{2}}}\mu$, on which Encke's table is founded, is generally proved in the books by a trigonometrical transformation. The present method is preferable when once the series for k^2 is regarded as known. This series occurs in Olbers' work, and it looks very much as if Encke's table had been originally formed in this way.

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Combining I. II. III., we find

$$\delta u_1 = -\delta u_0 \frac{S\eta^2}{2u}\left(\frac{u+c}{r b^2}+\frac{u+c''}{r'' b''^2}\right);$$

or if

$$\delta u_1 = -F\,\delta u_0,$$

$$F = \frac{S\eta^2}{2u}\left(\frac{u+c}{r b^2}+\frac{u+c''}{r'' b''^2}\right);$$

and we have the following conclusions:—

(1) The true value of u always lies between u_0 and u_1;

(2) $u = u_1 + (u_0 - u_1)\dfrac{F}{F+1}$ will be an approximate value of u.

The convenience of the process will be best illustrated by an example. That which I have selected is worked in different ways, as an example of the various methods, in Klinkerfues' *Theoretische Astronomie*, 1871, pp. 95–119. The number of successive hypotheses required to attain a solution is considerable, according to all the methods given.

I shall now give the computation of one hypothesis, and its improvement by means of the function F. The calculation of a second hypothesis, founded on the improved value, and the improvement of the value found in the second calculation are sufficient for accuracy to five places; but as the second calculation is similar to the first, I give only its results. The further proceedings do not fall within the scope of this communication.

(Throughout the calculations, G. L. stands for "Gaussian logarithm"—*i.e.* addition or subtraction logarithm.)

Comet II. of 1813:—

$t = 7{\cdot}55002$ April 1813.
$t' = 14{\cdot}54694$
$t'' = 21{\cdot}59931$

λ =	271°	16′	38″	β = 29°	2′ 0″
λ' =	266	27	22	β' = 22	52 18
λ'' =	256	48	8	β'' = 9	53 12
$\odot$ =	17	47	41	log R = 0·00091	
$\odot'$ =	24	38	45	log R′ = 0·00175	
$\odot''$ =	31	31	25	log R″ = 0·00260	

Whence we have for the Gaussian quantities

log M = 9·75799 log B = 9·98706
G = 113° 43′ 57″ log B″ = 9·86038

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$\log g = 9\,38029$	$\log b = 9{\cdot}75645$
$H = 109°\ 5'\ 49''$	$\log b'' = 0{\cdot}05028$
$\zeta = 44°\ 13'\ 9''$	$g \cos \phi = 0\,17147$
$\log h = 9{\cdot}81477$	$c = 0{\cdot}31365$
$\log A = 9{\cdot}22527$	$c'' = 0{\cdot}95443$

Assume for first hypothesis $u_0 = 0{\cdot}3$ (Encke's preliminary rule gives $0{\cdot}2984$). Then we have in the usual way

$u_0 + c = 0{\cdot}61365$	$u_0 + c'' = 1{\cdot}25443$
$\log (u_0 + c) = 9{\cdot}78793$	$\log (u_0 + c'') = 0{\cdot}09844$
$\log \frac{1}{b} = 0{\cdot}24355$	$\log \frac{1}{b''} = 9{\cdot}94972$
$0{\cdot}03148$	$0{\cdot}04816$
$0{\cdot}06296$	$0{\cdot}09632$
$\log B^2 = 9{\cdot}97412$	$\log B''^2 = 9{\cdot}72076$
Diff. $0{\cdot}08884$	Diff. $0{\cdot}37556$
G. L. $0{\cdot}34772$	G. L. $0{\cdot}52820$
$\log r^2 = 0{\cdot}32184$	$\log r''^2 = 0{\cdot}24896$
$\log r = 0{\cdot}16092$	$\log r'' = 0{\cdot}12448$
$\log r'' = 0{\cdot}12448$	
Diff. $0{\cdot}03644$	
G. L. $0{\cdot}31963$	
$\log (r + r'') = 0{\cdot}44411 = \log S$	
$\log S^{\frac{1}{2}} = 0{\cdot}22206$	$\log \kappa = 8{\cdot}53661$
$\log \kappa (t'' - t) = 9{\cdot}68426$	$\log (t'' - t) = 1{\cdot}14765$
$9{\cdot}46220$	$9{\cdot}68426$
$\log \mu = 0{\cdot}00020$	$\log S^{\frac{3}{2}} = 0{\cdot}66618$
$\log k = 9{\cdot}46240$	$\log \eta = 9{\cdot}01808$
$\log k^2 = 8{\cdot}92480$	$\eta = 0{\cdot}10435$
$\log A^2 = 8{\cdot}45054$	$\log \mu = 0{\cdot}00020$
Diff. $0{\cdot}47426$	
G. L. $0{\cdot}29672$	
$\log u_1^2 = 8{\cdot}74726$	
$\log u_1 = 9{\cdot}37363$	$u_1 = 0{\cdot}23639$

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And since u_0 was 0·3,

$$u_0 - u_1 = 0{\cdot}06361$$
$$\log (u_0 - u_1) = 8{\cdot}80353.$$

We proceed to calculate,

$$F = \frac{S\eta^2}{2u}\left(\frac{u+c}{rb^2} + \frac{u+c''}{rb''^2}\right),$$

noticing that all the logs we require are already on the paper.

$\log \frac{u+c}{b}$	0·03148	$\log \frac{u+c''}{b''}$	0·04816
$\log \frac{1}{b}$	0·24355	$\log \frac{1}{b''}$	9·94972
	0·27503		9·99788
$\log r$	0·16092	$\log r''$	0·12448
	0·11411		9·87340
	9·87340		
Diff.	0·24071		
G. L.	0·43785		
	0·31125		
$\log \eta^2$	8·03616	$\log u_1$	9·37363
$\log S$	0·44411	$\log s$	0·30103
	8·79152		9·67466
	9·67466		
	9·11686 = $\log F$		
G. L.	0·05341 = $\log (F+1)$		
$\log \frac{F}{F+1}$	9·06345	u_1	0·23639
$\log (u_0 - u_1)$	8·80353		0·007362
	7·86698	U_1	0·24375
$(u_0 - u_1)\frac{F}{F+1}$	0·0073618		

The ultimate value of u on the assumed data is $u = 0{\cdot}24384$, according to my repeated verifications. (Klinkerfues states that it is 0·24389; this difference may possibly arise from some further corrections being admitted in the data). Consequently by this simple calculation, based on the results of one

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ordinary computation, we have arrived within one unit in the fourth place: a far more expeditious approximation than that given by any process I have found in the books.

It will always be necessary to make a second computation for the purpose of a check. With reference to this we may observe that careful five-place work is well repaid in the second computation, though not in the first. But for the second value of F three places are sufficient, though we shall obtain five for illustration; in fact, the influence of the exactness of F in the second computation is so small that the result in this example is the same whether we recompute F or not. Proceeding in exactly the same manner as before, but employing $U_1 = 0{\cdot}24375$ instead of $u_0 = 0{\cdot}3$, we find

$$u_2 = 0{\cdot}24385$$

and therefore

$$U_1 - u_2 = -{\cdot}00010$$

Recomputing F, we find

$$\begin{aligned} \log. F &= 9{\cdot}12483 \\ G. L. = \log (F+1) &= 0{\cdot}05435 \\ &\hline 9{\cdot}07048 = \log \frac{F}{F+1} \\ \text{Num. log } (U_1 - u_2) &= 6{\cdot}00000 \\ &\hline 5{\cdot}07048 \end{aligned}$$

and therefore

$$(U_1 - u_2)\frac{F}{F+1} = -{\cdot}000012$$

and to five places

$$U_2 = 0{\cdot}24384.$$

A recalculation gave for u_2 the same value to the last place; consequently the performance of two complete calculations in the manner indicated above has here been sufficient for five-place purposes.

The successive results are now placed together.

	Corrections given by present method
$u_0 = 0{\cdot}3$	
$u_1 = 0{\cdot}23639$	$+{\cdot}00736$
$U_1 = 0{\cdot}24375$	
$u_2 = 0{\cdot}24385$	$-{\cdot}00001$
$U_2 = 0{\cdot}24384$	

I may notice briefly that a useful check is given by the first

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deduction from our investigation (p. 356), viz. that the true value of u lies between u_0 and u_1, the initial and final values of one of the ordinary calculations, as exhibited at p. 359. For instance, in the present case, the final number (U_2) lies between both pairs, u_0-u_1, and U_1-u_2.

But the value of u given by Klinkerfues (p. 97) as that which would be obtained, "setzen wir die Versuche bis zur grössten Schärfe fort," is 0·24389, which falls beyond the limits U_1-u_2 by four units of the last place. Here certainly either my theory was wrong, or my computation of U_1-u_2 was wrong, or Klinkerfues was wrong. I believe, after a good deal of troublesome verification, that Klinkerfues' value is wrong upon the data assumed.

I think there can be no question that the present method gets over one of the most troublesome and unsatisfactory parts of the computation of parabolic orbits better than any method in common use.

Observations of the Companion of α Canis Majoris with the Transit Circle of the U.S. Naval Observatory. By J. R. Eastman, Esq.

Date.	Object.	Seconds of Transit.	Micrometer Readings.	Δα	Δδ	*p*	*s*
		s		s	′	°	″
March 20	α *Canis Majoris*	11·362	33·070	0·622	0·502	49·3	11·8
	Companion	11·984	32·568				
,, 22	α *Canis Majoris*	10·965	33·235	0·631	0·415	55·0	11·1
	Companion	11·596	32·820				
,, 23	α *Canis Majoris*	10·910	33·235	0·644	0·420	55·2	11·3
	Companion	11·554	32·815				

One revolution of the micrometer screw = 15″·312.

The transit of α *Canis Majoris* was observed over *six* threads and the companion over *five* threads. On March 20 both stars were unusually steady, and the observations of the difference of declination were much better than on the other nights.

Of course these observations have but little value in fixing the position of the companion, and are given only to show that it *may* be observed on a meridian instrument of 8·5 inches aperture.

Naval Observatory, Washington,
March 25, 1878.

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English Observations of the Satellites of Mars, 1877.

The following communications relating to the English observations of the satellites of *Mars* are those referred to in the Report of the Council at the general meeting of February last, see p. 209.

Observations made at the Royal Observatory, Greenwich.

Outer Satellite.

Date.	h m	Sidereal Time. h m	Position Angle. °	Distance. Radii.	Distance. "	Observer.	Remarks.
Sept. 5	9 33	20 33	270		60 ±	M.	Rough estimation.
Sept. 5	9 50	20 50	240	4 or	59	M.	Estimation.
Sept. 5	10 33	21 33	230	3½ or	48	M.	Estimation.
Sept. 5	11 0	22 0	225 ±	4 ± or	59 ±	W.C.	Rough estimation.
Sept. 19	12 21	0 17	190° to 195°	2 or	28	M.	Very rough est.
Sept. 28	10 36	23 7	110° to 115°	2½ or	33	M.	Estimation.
Sept. 29	8 55	21 30	248° 31′		80″·4	M.	Measure.
Sept. 29	9 10	21 45	255	6 or	77	M.	Estimation.

Inner Satellite.

Sept. 28	10 21	22 52	280° to 285°	2¼ or	29	M.	Estimation.
Sept. 28	10 41	23 12	285	2½ or	33	M.	Estimation.

In reference to these observations Mr. Christie writes:—

1. My first view of the outer satellite (on August 31, 12^h) was obtained when I was ignorant of its calculated position, being in fact under the impression that it ought to be on the other side of the planet. The other observations were made with a general knowledge of the time of E. or W. elongation, but without any Ephemeris of position angles and distances, so that the observer did not know whether the satellite would be N. or S. of *Mars*.

2. We are quite satisfied of the existence of objects seen distinctly in the approximate positions noted. The motion of the object seen on September 5 for several hours was sufficient to prove it to be a satellite, as it approached *Mars* somewhat, whilst if it had been a star the planet would have approached it by 60″ or 70″ in one and a half hours.* In the case of the inner satellite, the motion was slightly *from* the planet in the course of 20^m, whilst for a star there would have been an *approach* of 6″. No red colour was noticed in the outer satellite. The planet was wholly or partly hidden when observing the satellites. The outer one was well seen when fully half the disk of *Mars* was uncovered.

* With *increase* of position-angle up to 360°, about.

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Estimated Positions of the Satellites, as observed at the University Observatory, Oxford.

	The Outer.			The Inner.			
Date. 1877.	Sidereal Time, Oxford. h m	Position Angle. °	Distance. "	Date. 1877.	Sidereal Time, Oxford. h m	Position Angle. °	Distance. "
Sept. 8	23 0	69	80				
Oct. 2	21 31	80	80	Oct. 2	21 40	80	32
Oct. 4	22 5	305	61·5				
Nov. 1	22 30	225	30±				

In reference to these Professor Pritchard writes that no Ephemeris of the Satellites was used or known at the Observatory.

Outer Satellite of Mars.

Dr. Wentworth Erck writes:

That on September 3, at $23^h\ 0^m$ G.S.T., he most distinctly saw the satellite at position 64°, and continued to watch it at short intervals for a period of two hours, during which time it had moved to position 55°.

That on September 8, at $22^h\ 35^m$ G.S.T., he again saw the satellite at position 78°.

That at this date no Ephemeris of the satellite had been published to his knowledge, and that the first Ephemeris known to him was that in *Nature* of September 13, which had not reached his hands on September 15, on which day he again kept the satellite under observation for a period of nearly four hours, during which the positions were as follows:—

	h	m		°
Sept. 15	21	10	G.S.T.	Pos. 257
„	23	30	„	„ 243
16	1	0	„	„ 238

That again he kept the satellite under observation from September 25, $22^h\ 20^m$ G.S.T., when the position was 264°, to September 26, $0^h\ 35^m$, when the position was 255°.

That he again saw the satellite on September 27, $23^h\ 0^m$, at position 69°, and yet again for the last time on October 2, at 24 hours, when the position was 64°; but it had then become so faint that he had exceeding difficulty in seeing it at all.*

That comparison of the satellite with two stars seen at different times, in the field along with the satellite and in nearly similar positions with regard to the planet, enabled him subsequently to estimate the diameter of the satellite at 13·6 miles, or $\frac{1}{300}$th of the diameter of the planet, assuming the planet and the satellite to reflect light alike.

This satellite was more conspicuous than *Enceladus*, and was on one occasion seen distinctly and steadily with an equilateral triangular aperture whose side was 6 inches.

* The satellite was never seen at much less than two diameters of the planet from the limb.

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The inner satellite was never seen; but unfortunately during the first half of September, that is to say during the most favourable period, despair of success with the inner satellite concentrated attention on the outer one.

The observations were made with that 7½-inch object glass which Alvan Clarke first sent the late Mr. Dawes. At all times an hexagonal aperture was employed, with single lenses as eye-piece, giving powers of 250 and 300.

Mr. H. C. Key writes:

"I first saw the outer satellite of *Mars* on the night of September 19, at about 9^h 30^m, in the position assigned in the Ephemeris published in *Nature*. I used an 18-inch silvered glass equatoreal Newtonian. With an ordinary negative eye-piece, and a bar in it hiding the planet, I could hardly do more than glimpse the satellite occasionally, owing to the scattered light from *Mars*; but with a concave eye-lens giving nearly the same power, 180, I could see the satellite pretty distinctly notwithstanding the brilliancy of the primary. Two or three nights afterwards—I was prevented making a note at the time as several friends were with me—I again saw the satellite with the same eye-piece, having myself calculated its approximate position in the absence of an Ephemeris. I have no doubt whatever in my own mind that the object was a satellite of *Mars*. I looked long and carefully for the inner satellite, but failed to find it.

"Were the opportunity to come over again, I have little doubt that, with a diminished aperture on the telescope—12 inches instead of 18—and a better prepared method of hiding the primary, I should be able to measure the distance and position of this satellite without much difficulty.

Mr. Brett writes:

"I enclose a tracing from the entry in my observing book, which I think comprises all the data I can offer as regards distance and position angle, being only eye estimates, and their accuracy being marred by the bar in the field, which was found necessary in order to conceal the disk of the planet. My bar, however (a minute slip of paper), was partially transparent, and enabled me to judge the distance in semi-diameters with tolerable accuracy.

"You will notice that the recorded observations extend over one hour and twenty minutes, but that only the outer satellite persists through the whole interval. The righthand one of the dots marked in my diagrams was only perceived a little before 10^h, and as its place did not at all accord with the ephemeris of the inner satellite as supplied to me, I have nothing to remark about it, except that, with equally good definition, it was not visible at 9^h 20^m, but I thought it too *far* from the planet for the inner satellite (?).

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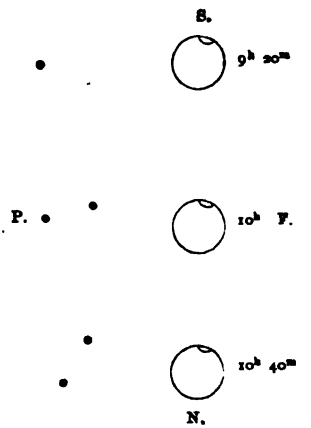
"As regards the outer, the observations were quite free from doubt in my own mind. The point of light was exceedingly minute, but was *not* lost to sight at any time during the interval named.

"I had sought for the satellites on many evenings previously in vain, but the plane mirror had had its silver a good deal worn off. Early in September I procured from Mr. Browning a new plane mirror, and likewise from Greenwich an Ephemeris.



The Ephemeris for September 15 gave greatest elongation of outer satellite at 9h. On that occasion my new plane mirror mounted, and on placing the telescope on *Mars* I very soon perceived the minute point marked in diagram.

"The second minute point was of about the same brightness as the first. Both died out of sight shortly before 11.h

"Aperture of telescope 9′·1, power 380 (by With-Browning.)"

Mr. Marth has compared the foregoing observed places of the outer satellite, and also those of Mr. Common given in the January No., with places computed from approximate elements deduced from some of Professor Asaph Hall's observations.

Greenwich Observations.

				Obs.			Comp.	
		h m		Pos. °	Dist. ″		Pos. °	Dist. ″
1877	Sept. 5	9 33	G.M.T.	270	60	est.	245·8	82·9
		9 50		240	59	,,	244·6	81·4
		10 33		230	48	,,	241·4	76·9
		11 0		225 ±	59	,,	239·1	73·3
	19	12 21		190° to 195°	28	,,	182·9	28·6
	28	10 36		110° to 115°	33	,,	118·9	34·9
	29	8 55		248° 31′	80·4	meas.	249·11	73·7
		9 10		255°	77	est.	248·0	72·8

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Oxford Observations.

	h	m		Obs. Pos. °	Obs. Dist. ″	Comp. Pos. °	Comp. Dist. ″
Sept. 8	23	0	Oxf. Sid. T.	69	80	71·7	85·5
Oct. 2	21	31		80	80	86·3	64·1
4	22	5		305	61·5	253·0	72·3
Nov. 1	22	30		225	30 ±	?	

Dr. Erck's Observations.

	h	m		Obs. °	Comp. °
Sept. 3	23	0	G. Sid. T.	64	65·6
	? 1	0		55	55·6
8	22	35		78	74·2
15	21	10		257	256·5
	23	30		243	247·2
16	1	0		238	267·1
25	22	20		264	266·3
26	0	35		255	322·4
27	23	0		69	72·9
Oct. 2	0			64	76·3

Mr. Brett's Diagrams.

Sept. 15	9	20	G.M.T.	?	257·2	6·8 rad.
	10				254·5	6·9
	10	40			251·9	6·9

Mr. Common's Observations.

	h	m		Obs. °	′	Comp. °
Sept. 11	9	52	G.M.T.	270		291·3
15	10	15		253	48	253·53
	10	20		252	44	253·20
	10	41		251	2	251·83
	10	47		250		251·5
	10	50		250	40	251·24
	12	43		244	28	243·28
	12	51		240	40	242·62
	13	5		243		241·5
16	12	43		273		273·0
	12	47		269		272·5
	12	50		273	30	272·07
	12	57		270		271·2

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	h	m	Obs. °		"	Comp. °	"
	13	5			63·2		62 8
	13	44	266	4		266·28	
	13	50			72·1		69·8
	13	58	263	4		264·99	
	14	0	263	36		264·81	
	14	5	264	8	75·6	264·38	71·9
21	8	34	62	8		32·93	
24	8	49	240	30		243·84	
	9	8	236	30		242·24	
28	11	23	111			131·5	
29	9	28	244	30		246·69	
	9	33	245	45		246·30	
	10	29	242			241·64	
	10	36	239	3		241·18	
30	10	28	269	30		271·51	
	10	38	267	30		270·60	
	10	52	267	0		268·93	
	11	0	266			268·14	

Note on the Table, p. 70 of Vol. II. Dun Echt Observatory Publications. By Lord Lindsay.

There is in the description of the table an ambiguity, which may be cleared up thus:

In taking out N from the tables the sign is to correspond to the sign of the cosine of the Hour Angle for Latitude North, and the opposite sign for Latitude South

The sign of M is immaterial, as Z is always positive.

780412.
Brook Street.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. *May* 10, 1878. No. 7.

LORD LINDSAY, M.P., President, in the Chair.

Maxwell Hall, Esq., M.A., Jamaica; and

James L. McCance, Esq., Rathfern, Rayner's Road, Putney Hill;

were balloted for and duly elected Fellows of the Society.

On the Supposed Influence of a Mass of Brickwork upon the Errors of a Transit Instrument in its Neighbourhood. By John J. Plummer, M.A.

During a considerable portion of the years 1875-6 I was engaged in work that necessitated a strict watch upon the errors of the small transit instrument which serves as an adjunct to the Equatoreal of this Observatory for the determination of true time. It was my practice to find the level error upon each evening of observation, and to employ it in the reduction of that night's work; while the azimuth error was determined as often as was deemed necessary, or as circumstances permitted. The method employed for ascertaining the latter was generally the observation of a polar and a low southern star, and the errors adopted for the reductions are the means of two, three, or more such determinations. It was soon found that both errors, but particularly the azimuth, were very fairly steady for considerable intervals, notwithstanding that the stone piers which support the instrument rest on arches at a height of not less than 50 feet from the ground; in fact, but for this fortunate circumstance this mode of treatment would have been hardly admissible.

To economise space, in place of the actual individual results obtained, a list of the mean adopted errors of azimuth is here appended, together with the limiting dates of the period to which they are supposed to apply, and likewise the means of corresponding groups of errors of level.

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Limiting Dates for each group.	Mean adopted Azimuthal Error.	No. of Determinations on which it depends.	Mean Level Error for the like period.	No. of Determinations on which it depends.
1875 Aug. 11—Aug. 12	− 1″·82	1	+ 4″·96	2
Aug. 14—Aug. 16	+ 1·84	2	+ 5·69	3
Aug. 20—Oct. 2	+ 0·26	3	− 3·49	12
Oct. 5—Oct. 22	+ 0·93	4	− 2·26	9
Nov. 8—Nov. 25	+ 4·58	3	+ 1·99	5
Dec. 7—Dec. 20	+ 5·62	2	+ 6·06	2
Dec. 22—Jan. 5	+ 7·83	3	+ 7·96	3
1876 Jan. 18—Jan. 22	(+ 9·80)	0	+ 7·89	2
Jan. 25—Feb. 11	+ 11·76	3	+ 6·14	6
Feb. 22—Feb. 24	+ 10·44	2	+ 5·85	3
Feb. 29—Mar. 10	+ 8·55	3	+ 4·00	5
Mar. 11—Mar. 29	+ 6·00	6	+ 0·44	10
Apr. 2—Apr. 8	+ 6·50	2	− 0·86	4
Apr. 21—Apr. 29	+ 3·30	3	− 3·43	5
May 1—May 9	+ 1·89	2	− 9·27	4
May 29—May 30	(− 0·06)	0	− 8·47	2
June 16—June 28	− 2·01	2	− 13·76	7
July 26—Aug. 2	− 5·81	1	− 3·50	2
Aug. 9—Aug. 17	− 1·00	4	− 4·59	6
Aug. 21	− 4·41	1	− 5·52	1
Aug. 25—Sept. 9	− 12·17	2	− 9·60	7
Sept. 14—Oct. 5	− 4·55	2	− 5·52	7
Oct. 7—Oct. 14	− 3·76	1	− 1·54	5
Nov. 7—Nov. 17	− 1·14	1	− 4·50	3

N.B. On June 27, the level error having become troublesomely large, the screws were altered in order to diminish it; but this alteration does not appear to have affected the azimuth error. After the alteration the level errors were more unsteady, and a month later the screws were tightened to ensure greater fixity to the instrument. This time, owing probably to the greater force used, the Y appears to have shifted in azimuth sensibly.

A casual inspection of the above azimuth errors is sufficient to indicate a marked and regular fluctuation, of which the maximum and minimum coincide closely with the periods of greatest cold and heat respectively, and for which no sufficient reason immediately suggests itself. It is to be remarked, however, that about 14 feet W.S.W. of the western pivot of the transit is situated the pier of the Equatoreal, which, with the wall surrounding it, forms an immense cylindrical mass of nearly solid brickwork 9 feet in diameter, built up from the ground. It follows, therefore, that

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the conditions of radiation from the transit supports in this direction is materially different from that in every other, and that the temperature of the pier of the Equatoreal would not only change much more slowly than the surrounding walls of the transit-room, but would be subject to much less extreme degrees of heat and cold. This supposition is sufficient to explain the general tendency observed, although the magnitude of the variation is certainly greater than would have been expected. That this is the true explanation is strongly supported by the consideration of the less steady level errors, which are positive in the winter months and largely negative in the summer, precisely as would be the case if the explanation is correct, and moreover exhibit maxima and minima almost synchronising with the more trustworthy azimuthal results. As it must be conducive to accurate observation that all known causes of change of position of meridional instruments be eliminated, I have been induced to call attention to these results, in order that the surroundings of more important instruments may be made as symmetrical and homogeneous as possible.

Orwell Park Observatory,
1878, *March* 1.

List of Known Accordances between Cometary and Observed Meteor Showers. By Prof. A. S. Herschel.

This list of comet and meteor shower accordances supplies a key to the outline list presented two years ago in the Report of the Council of the Astronomical Society (vol. xxxvi., p. 220), in which the meteor showers, regarded as having close or distant affinities in the positions of their orbits to those of certain comets (enumerated in the list), were not, on account of the great length of such a table, particularly recorded and described. The former list contains a column (Column 9 in the present list) in which the degree of accuracy of the supposed accordance of each correlated meteoric shower, both in time and in position of the radiant-point, was represented by certain letters and accents, of which the use was explained in the introduction to the Table,* and they are here reproduced for identification of the meteor showers referred to in the list. But an additional column of the same nature is now supplied (No. 8 in this list), giving the same characters more accurately, and describing the accuracies of agreement of several new meteor showers which are noticed for

* A *a*, B′ *b*′ C″ *c*″ ? ; denote distances of 3, 6, 9, 12, 15, 18, and over 18 degrees (by the letters and sign of interrogation), and similar numbers of days (by the accents, comma, and semicolon), from exact accordance with a comet shower; parentheses being used to enclose them, thus (A), when the meteor shower, and brackets, thus [A], when the comet appulse is of longer duration than 6 days.

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the first time as presenting resemblances either to the comets formerly enumerated or to some equally well attested ones of which the indications of pretty close coincidence have only recently been observed. The order of succession, and the total number of the accordances differs very little from those of the earlier list, and the consecutive numbers of that list (with those of a few similar comparisons published during the past few years in the Reports of the British Association) are given in the first and following columns of this Table.

The arrangement of the Table in its remaining columns and remarks calls for very little explanation; the representation of the cometary radiant-points, and of their Earth distances, at "Node" or "Appulse" (calculated by Dr. Weiss where W. is appended to them), having already been sufficiently elucidated in the above quoted introduction of the former list, and the times and positions, and authorities of the meteor radiant-points, with the letters and numbers that are used to denote them, being those which have been frequently described elsewhere by Mr. Greg in his published general Lists of Meteor Showers*. The single letter T. denotes showers in Captain Tupman's list†; Schm., showers in the list of radiant-points given by Dr. Schmidt in the *Astronomische Nachrichten*, No. 1756; D. observations by Mr. Denning; and C. by Mr. H. Corder. The letters T., S., W. following Mr. Denning's showers indicate positions of radiant-points which he has detected by reductions during the past year of the long lists of meteor-tracks—in Captain Tupman's Catalogue (T.), Schiaparelli's Catalogue of the Italian Luminous Meteor Association for the year 1872 (S.), and Dr. Weiss' Austrian Observations of Shooting Stars in the years 1867–74 (W.)—from which most of the new, and some of the closest and apparently most satisfactory accordances of the present list, have been derived.

* These *Notices*, vol. xxxii., p. 348. The initials G.H., H.N., S.Z. denote the catalogues of meteor showers derived from original observations by Greg and Herschel (*British Association Reports*, vol. for 1868), Heis and Neumayer (in the southern hemisphere, *Ibid.*), and Schiaparelli and Zezioli (in the German translation, by Dr. von Boguslawski, of Professor Schiaparelli's work on shooting stars).

† These *Notices*, vol. xxxiii., pp. 298–305, March 1873; here referred to as it was reprinted with some small subsequent alterations in the volume for 1874 of the *Reports of the British Association*, and in a pamphlet circulated by the British Association in that year, containing all the observations of meteor-tracks mapped and recorded by Captain Tupman in the Mediterranean during the years 1869–71.

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 299.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☄'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2) °	(3) °	(4)	(5)	(6)	(7) °	(8)	(9)	(10)
	3	1	1	...	Jan. 5	194	+24·5	1792 II ☄ (W.)	+0·07					
					Jan. 11, 12	183	+28	S.Z. 5	...	6	11·5	*b*,		
					Jan. 4–31	180	+35	T. 4	...	(0)	12·5	(C)		
					Jan. 1–25	183	+36	MG_1 (G.H.)	...	(0)	13	(C)		
			1*a*	...	Jan. 6	187	−22	1860 IV ☄	−0·045					
					Jan.–Feb.	188	−26	D.T.	...	(0)	4	(*a*)	...	8↓*e*; near *β Hydræ*.
	2	...	2	...	Jan. 20	128·5	−28·5	1840 I ☄ (W.)	−0·04					
					Jan. 5	145	−25	T. 7	...	15	16·5	*c*"		
					Jan.	145	−40	Δ_2 (H.N.)	...	(0)	20	(?)		
	15	..	3	(9)	Jan. 16	60	+40	1746 υ	(+0·07)					
					Jan. 28	67	+25	S.Z. 19	...	12	17	*c*'	?'	
					Dec. 20?–Feb. 6	65	+20	AG_1 (G.H.)	...	(0)	21	(?)	(?)	

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Mon. Not. R.A.S., vol. xxxvi. p. 22—.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☄'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
1a	...	...	4	...	Jan. 19	210	−15	1759 III Ω	(−0·05)					
					Feb. 8	231	−21	Do. (Perih.)	(−0·07)					
					Jan. 5, 11	210	− 6	T. 6	...	8	9	B′		
					Feb. 3–10	219	−23	T. 12	...	(0)	11·5	(b)	(B)	
					Feb. 17	218	−13			9	15+	c′		
					Jan.–Feb.	204	−10	D.T.	...	(0)	7·5	(B)	...	In *Libra*. 10 meteors.
					Jan.–Feb.	210	−13	D.	...	(0)	2	(A)	...	5 meteors (active in April at 203–8, 23 meteors).
			4a	(6a)	Jan. 20	256	+20	1672 ♂	+0·04	...	...	...	...	☄ 1863 V ♂ and 1810 ♂ (Jan. 24, 29, 272+25, 277+21), are near these; and form with (6a, *see* the note) a remarkable ☄ group.
	✔				Feb. 2	261	+23	1857 I ♂ (W.)	+0·03					
					Jan.–Feb.	251	+23	D.	...	(0)	5·5	(a)	...	11↓ˢ, in *Herc.* 1877; and confirmed by other observations.

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2	...	34	5	...	Jan. 27	135	+25	1833 υ	(+0·04)				
					Feb. 12	144	+24	1833 θ	−0·21				
					Jan. 1–Feb. 9 (max. Jan. 28–31)	135 to 140	+40	$M_{1,2}$ (G.H.)	...	(o)	16	(*o*)	(?)
					Jan. 31	134	+40	S.Z. 23	...	3	16	*c*	?
					Feb. 3	153	+21	S.Z. 26	...	7	14·5	C′	C
					Feb. 13	133	+26	S.Z. 32	...	1	10	*b*	B
	5	...	6	...	Jan. 29	208·5	−31	1718 θ (W.)	+0·04				
					Feb. 3–10	198	−22	T. 11	...	(5)	12	C,	
					Jan.–Feb.	213	−32	D.T.	...	(o)	3	(A)	...
3	6	...	6*a* (4*a*)		Feb. 14	266	+ 9	1699 I *c*	+0·117				
					Feb. 13 ('69)	260	0	T. 16 (suspected)	...	1	10·5	*b*	*b*
4	7	2	7	...	Feb. 18	211	+ 9	1797 θ	+0·27				
					Feb. 13	205	+ 4	T. 8	...	5	8	B,	B,
					Mar. 2, 3	209	+18	T. 22	...	12	9+	*b*′	

8 meteors, two showers (209−27 and 218−37).

g[a] 1858 IV and 1799 II, Feb. 13–15, are close to this position, at 272+12, and 264+17. (*See* the note above, 4*a*.)

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☽'s Node (or Appulse); +without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
5	10	...	8	(10)	Feb. 23	285	− 8	1596 ♉	+1·4					
					Feb. 26	283	− 4·5	1845 III ♒	+0·06					
					Feb. 10 ('70)	290	−12	T. 14 (not act.)	...	13	7	B″	B″	[T. 28, Mar. 7, 270−22, is near. *See* No. 10.]
										16	10	*b*″		
1	15	...	9	(3)	Feb. 25	33	+33·5	1746 ♓	−0·03					
					Mar. 8	30	+28·5	1746 ☋	(−0·01)					
					Mar. 10	32	+31	1231 ☋	(+0·06)					
					Feb. 20–Mar. 1	33	+36	D.W.	...	(0)	3	(A)	...	10 meteors; active shower. A_8 (Heis, and G.H.), Mar. 1–15, (??) in list (*a*), are too distant.
					Feb.–Mar. 12	28	+35	D.S.	...	(0)	5	(*a*)	...	16 meteors, at β *Trianguli*.
7	11	...	10	(15)	Mar. 8	275	−38	1590 ♌	−0·30	...	...	...	...	1506 ♌, Feb. 6, +0·43, 266·5−37.
				(8)	Mar. 7 ('70)	270	−22	T. 28 (after 3 A.M. only)	...	1	16	*c*	*c*	1877 I ♌, Mar. (15–) 27, −0·185, 273−40; Λ, (H.N.), April, 280−38. *See* 15.

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6	8	3	1	...	Mar. 1	250·5	−12·5	1864 V ♂	+0·115					
	9				Mar. 16	249·5	+ 1	1862 IV ♂ (W.)	−0·013					
					Mar. 2–7	235	−15	T. 23	...	1	15	C	*c*,	2 close showers; bright on Mar. 7, '69.
					Mar. 3–25	247	− 3	SZ (G.H.)	...	(0)	9·5; 4	(*b*; *a*)		
					Mar. 7 ('70)	246	+ 0	T. 27	...	9	3·5	*a'*	C, *a'*	Suspected at 241 + 8, Mar. 7, 1869.
					Mar. 14, 15 ('69)	266	+ 6	T. 21	...	1	18	*c*	...	
					Mar. 2–7	246	+16	T. 19	...	9	17	*c'*	C'	2 close showers; bright on Mar. 7, '69.
9	13	5	12	(22)	Mar. 16	207	−48·5	1683 ♌ (W.)	+0·03					
					Mar.	192	−38	H_1 (H.N.)	...	(0)	14·5	(C)	(C)	
					Mar. 11–19 ('69)	203·5	−30·5	T. 17	...	(0)	16·5	(*c*)	(*c*)	2 close showers.
8	14	...	13	...	Mar. 18	312·5	+21·5	1763 ♌ (W.)	+0·02	...	...	...	...	♂ 1854 V, 1845 I, Feb. 13, 25, are nearer WZ, in position; ♂ 1580, 1784 II, Apr. 12, 26, have too great R.A.(335°); six comets in a group.
					Apr. 24	319	+19	1790 III ♌ (W.)	−0·06	...	...	...	...	
					Mar. 15–Apr. 20	305	+37	WZ (G.H.)	...	(0)	18	(*c*)	(*c*)	(Apr. 21, '72, 310 + 44; Denning).

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Reference Nos. Brit. Ass. Rep. 1875, p. 229.	Reference Nos. This List.	Reference Nos. Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☄'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
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						°	°				°			
10	12	4	14	...	Mar. 19	179	−26	1556 ☌	+0·20					
					Mar. 25	182·5	−28	1264 ☌	−0·02					
					Mar.	174	−30	Δ_4 (H.N.)	...	0	6·5	(B)	(B)	
			15	(10)	Mar. (15–)27	273	−40	1877 I ☌	−0·185	...	...	...	...	(*See* No. 10).
					Apr.	280	−38	A_1 (H.N.)	...	(4)	5·5	(a,)		
	10	...	16	(19)	Mar. 23	308	+12	961 ☌	+0·27					☄ 1763, 961, Mar. 18, 23, and 1857 V, 1825 I, Ap. 4, 9, are a close group about 308+15 (±6°). *See* 19.
✓	17	...	...	...	Apr. 4	302	+11	1857 V ☌	−0·28					
					Mar. 1–19, Apr. 19–22	301·5	+12·5	Denning 1877	...	(4)	6·5	(B,)	...	14↓°; observed in 1877.
										(16)	1·5	(A")	...	
					Apr. 1–22	304	+12	D.S.W.	...	(0)	2	(A)	...	15↓°; reduced and observed in 1877.

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✓ 11	16	...	17	(21)	Apr. 11	231·5	+27	1847 I ♂	−0·95	...	...	...	...	♂ visible in daytime. The agreement, suspected by Schiaparelli, is imperfect in perih. dist. Comp. No. 37.
					Apr. 13	231	+27	S.Z. 54	...	2	0·5	A	A	
					(Mar. 27–May 22)	234	+29	S.Z. (44, 48, 54, 65, 67, 70) aver.	...	(0)	4	(*a*)	A′ B″	6 showers, all quite close together (230 to 238, +24 to +36).
					Mar. 12–Apr. 30	223	+40	MG_2 (G.H.)	...	(0)	14·5	(C)	(*c*)	
					Apr. 11–30 ('73–'76)	241·5	+24·5	D.	...	(0)	9·5	(*b*)	(*b*)	13 meteors ('74–'76), '*Coronids*.'
					Apr. 12–June 30	235 to 240	+25	$Q_{1,2}$ (G.H.)	....	(1)	8·5	(B)	(B)	(Corrected pos., 1876–77, 235+25).
					Apr. 1–13	235	+25	D.S.	...	(0)	4·5	(*a*)	...	19 meteors, '*Coronids*.'
✓ 14	18	8	18	...	Apr. 15	116·5	−36	1830 I ♂	−0·08					
					Apr.	126	−42	Γ_4 (bis, H.N.)	...	(0)	10	(*b*)	(B)	
					Mar.	125	−38	Γ_4 (H.N.)						
12	...	...	19	(16) (23)	Mar. 26	296	+ 1·5	1743 II ♂	−0·30	...	...	...	...	(*See* 16). ♂ 1790 III, Apr. 24, is at 319+19. *See also* No. 23.
					Apr. 15	307	+ 4	1808 III ♂	−0·27					
					Mar. 25–Apr. 30	290	−10	OZ (G.H.)	...	(0)	12·5	(C)	(C)	
✓	19	7	20	...	Apr. 20	270·5	+32	1861 I ♂	+0·01					♂, 270·5+33·5 (Schiap.); 265+35 (Weiss?)
					Apr. 19–21	277	+34	QH_2 (G.H.)	...	0	6·5	B	...	The '*Lyrids*'; [their exact radiant-point perhaps uncertain (?)].
					Apr. 20, '69	267	+35	Karlinski	...	0	4·5	*a*		
					Apr. 20–22 '78	272	+32	D.	...	0	2	A	...	13 ↓[a] near the radiant-point.

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						°	°				°			
	20	9	21	(17)	Apr. 22	255·5	+27·5	1748 II ♉	−0·11					
					Apr. 23	250	+40	S.Z. 60	...	1	14	C		
					Apr. 25	260	+24	S.Z. 63	...	3	6	*a*	...	Agrees fairly with the *g* (Schiaparelli).
					Mar. 15–Apr. 23	268	+25	QH_1 (G.H.)	...	(0)	11	(*b*)		
					Apr. 1–13	255	+27	D.S.	...	9	0	A	...	9 meteors.
15	13	6	22	(9)	Apr. 23	203	−32·5	178 ♌	+0·22					
					Apr.	194	−30	H_2 (H.N.)	...	(0)	8	(B)	(B)	
12	...	...	23	(19)	Apr. 21	288·5	+ 5	1844 II ♉	−0·08					
13	17	10	...	...	May 1	296·5	+13·5	1853 II ♓	−0·07					
					Apr. 19–23	287	+22	D. 1872–78	...	0	17	*a*	...	A good centre; position observed and deduced from various catalogues.
					May 2 ('70)	285	+12	T. 34	...	11	8	B'	B'*b*	And Apr. 19–27, 286+5, 12 ↓° observed in 1877, and confirmed by other observations. (Denning.)
						298	+ 5	T. 35	...	1	8	B	*b*'B	

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17	...	...	24	...	Apr. 12	215	−28	1737 I Ω	(−0·13)				
					May 8	235	−15	1737 I ♌	−0·48				
					May	223	−12	Schm.	...	(o)	12·5	(C)	(*c*)	(T. 32, Apr. 27–28, 230+5, is connected with later showers in higher Decl.)
					Mar. 20–May 29	227	− 5	SG_2 (G.H.)	...	(o)	13	(*b*)	(*b*)	
16	21	12	25	...	May 1	334·5	−16	837 I ♌	+0·03					
					May 4	337	0	1835 III ♉	(−0·06)	(Halley's Comet)				
					Apr. 30–May 2, 3	326	− 2·5	T. 33 ('70, 71)	...	o 1	15·5 11	*c* *b*	C *b*	Bright showers, especially April 29 1871, and May 2, 3, 1870.
18	22	11	26	...	June 10	273·5	+ 0·5	1618 III ♓	+0·10					
					June 10–13	273	− 3	D.S.	...	o	3·5	*a*	...	11 meteors, Ital. Cat. 1872.
					June	282	− 3	Schm.	...	(o)	9	(B)	(B)	
				(33)	June	266	−12	Schm.	...	(o)	14	(*c*)	(C)	Active also in July and August.
					June	269	−11	O_1 (H.N.)	...	(o)	12	(*b*)	(*b*)	
19	...	13	27	(28)	June 14	338	+57	1781 I ♓	−0·19					
					May 26–June 13	337	+59	D.S.	...	(1)	2	(A)	...	10 meteors, Ital. Cat. 1872.
					May 1–31	325	+55	B_1 (Heis)	...	(14)	7	(B″)		
					June	333	+42	B_2 (Heis)	...	(o)	16	(*c*)	(C)	

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						°	°				°			
20	23	14	28	(27)	June 24	312·5	+60·5	1850 I ☄ (W.)	+0·065					
					May 26–June 13	312	+63	D.S.	...	(11)	2·5	(A')	...	5 meteors, Ital. Cat. 1872.
					June 11–July 11	315	+60	B_1 (G.H.)	...	(0)	1·5	(A)	(A)	
					July 1–15, 16–31	315	+54	B_2 (Heis)	...	(7)	7	(B')	(B')	
						320	+70	B_4 (Heis)	...	(21)	10	(*b*;)		
					July 8	288	+64	S.Z. 89	...	14	11	*b*''		
					July 13	338	+65	S.Z. 94	...	19	12·5	C;		
			29	...	June 20	8	+ 5	1864 II ♉	(0·00)					
21	24	15	...	...	June 27	12	+ 6	1864 II ☄	−0·05					
					July	7	+ 4	Schm.	...	(4)	5·5	(a_1)	(A)	[And Aug., 3+1.]
					July	18	0	Schm.	...	(4)	8·5	(B_1)	(B)	
					July	0	+17	Schm.	...	(4)	16·5	(c_1)	(C)	[And July, 20−11; (4^d), 19°, (?,).]
22	...	...	30	(31)	June 25	348·5	+28	1822 IV ☄	+0·14					
					July	345	+25	Schm.	...	(5)	4·5	(a_1)	(*b* C)	
					July 18	342	+23	S.Z. 100	...	24	7	B;	B''*b*	

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23	...	...	31	(30)	June 30	342	+14	1822 III ♂	+0·11				
					July 13	349	+12	1770 II ♂	−0·09				
					June 1–13	343	+16	D.S.	...	(17)	2	(A")	...	8 meteors, Ital. Cat. 1872.
					June	335	+10	Schm.	...	(0)	8	(B)	(*b*)	And July 5–28, 335+7; (5^d), 10°, (*b*,). 20–31, 334+1; (20), 15, (*c*;).
					June 28	338	+13	T. 38	...	2	4	*a*	*a*	And July 1–6, 337+1; 1^d, 14°·5, C.
					June 29–Aug. 24	330–45	+14	T_1 (G.H.)	...	(0)	(0)	(A)	(*a*)	'*Pegasids.*'
27	...	18	32	(35)	July 8	39	+45	770 ♂	+0·20				
					June 1–13	35	+47	D. S.	...	(25)	3	(A;)		6 meteors. Ital. Cat. 1872.
					July 6–20	36	+47	D. 1877	...	(0)	3	(A)		6 meteors, not *Perseïds.*
24	27	16	33	...	July 8	276	−21·5	1770 I ☋	(+0·02)	...	...	...	...	Until ☋, Nov. 29, 257·5−21 (−0·025). Lexell's Comet.
					Aug. 6	283	−20	1770 I ♂	−0·22					
					June 29–July 6	283	−13	T. 36	...	(2)	10·5	(*b*)	*b*	Average of 3 close radiants.
				(26)	July (& Aug.)	266	−12	Schm.	...	(0)	13	(C)		
					July 18–Aug. 31	285	−25	Schm.	...	(0)	5	(*a*)	(*a*, *b*, *b*)	Average of 3 close radiants. And Sept., 282−22; (0^d), 5°, (*a*).
25	28	...	34	...	July 29	175	+71	1737 II ♂ (W.)	−0·025					
					End of July	165	+62	Schm.	...	(0)	10·5	(*b*)	(*b*)	Confirmed by Schiap. (S.Z. 118), and by G. & H. (V) more distantly.

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						°	°				°			
26	25	17	35	...	July 23	262·5	−33	568 Ω	(−0·01)					
					Aug. 5	259	−36	568 ☌	−0·06					
					July	258	−20	O_2 } Neum.	...	(0)	13	(C)	(C)	Fireball, July 25, '76, England, 258−24 (near Antares).
					Aug.	250	−35	A_8 } Neum.	...	(0)	7·5	(B)	(B)	
					Aug.	266	−42	Schm.	...	(0)	8	(B)	(*b*)	And July, 245−30, (o^d) 17°, (o_c).
27	30	18	...	...	July 25	49	+45·5	1764 ☌	−0·11					
			36	(32)	Aug. 10	43	+57·5	1862 III ☌	+0·02					
					Aug. 12	43·5	+53	1870 I ☌	+0·03					
					July 12–20	47	+45	D. 1877	...	(5)	2	A,		5 ↓ᵉ, leaving streaks, not *Persëids*.
					Aug. 7–12 (10, max.)	44	+56	...	...	0	1·5	A	A	'*Persëids*.'
28	29	...	37	...	Aug. 12	299	+80	1853 III ☌ (W.)	−0·69					Large ☄, visible in daytime.
					July 24–Aug. 11	315	+87	S.Z. 115, 135, 143	...	(1)	7	(B)	B, A' *b*	Average of 3 close radiants. Agreement in perih. dist. is imperfect.
					July 16–Aug. 31	315	+84·5	$N_{12, 13, 14}$ (Heis)	...	(0)	6	(*b*)	(B)	Average of 3 close radiants. The agreement, supposed by Weiss, is doubted by Schiaparelli.
					July 28–Sept. 10	359	+89	$N_{12, 13}$ (G.H.)	...	(0)	10	(*b*)	(B)	
					Aug. 10, 11, 22	270	+83	T. 52	...	(0)	5	(*a*)	*a' o*	Average of 3 radiants observed by Denza and Tupman.

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			38	...	Aug. 9	32	−18·5	1877 II ♌	+0·30				
					Aug. 1–12	26	− 6	Schm.	...	(o)	14	(C)	
29	31	19	38*a*	(39)	Aug. 10	40·5	−13·5	1852 II ♌ (W.)	+ ·013	•			
				(42)	Aug. 11	48	− 8	1827 II ♌	−0·16				
35	32	...	...	...	Aug. 26	65	−22	1558 ♌ (W.)	−0 11				
					Aug.	55	−18	Schm.	...	(o)	10	(*b*)	(*c*)
30	33	21	39	(38*a*)	Aug. 7	41	+11·5	1862 II ♉	(−0·025)				
					Aug. 19	47·5	+13	1862 II ♍ (W.)	+0·03				
34	...	...	...	...	Aug. 22*	57	+21	1864 II ♉	+0·03				
					Aug. 10	47	+18	S.Z. 140	...	[o]	6·5	[B]	B
					Aug. 10	48	+19	Denza (T. 48)	...	[o]	8	[B]	B
					Aug. 11 & 12	41·5 / 46·5	+24	Weiss (T. 48)	...	[o]	12	[C]	*b* Two close radiants.
					Aug. 4, 22, 28	40	+30	T. 48	...	[o]	18	[(?)]	... Average of 3 radiants.
					Aug. 3–15	55	+26	RG_1 (G.H.)	...	[o]	17·5	[*c*]	*c*
					Aug. 3–12	55	+ 7	Schm.	...	[o]	12·5	[C]	(C*c*)
				(42)	Aug.	53	+ 1	Schm.	...	[o]	14	[(C)]	(C*b*)
					Aug. 20·25	53	+ 1	T. 65	...	[o]	14	[C]	(C) *b'*
					Sept. 3–30	51	+14	Schm.	...	(12)	9	(B")	

* The date and apparent place (Sept. 2, 57 + 1) of this cometary radiant-point was given erroneously in former lists.

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 390.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☌'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
31	34	22	40	...	Aug. 14	3·5	+38·5	1780 II ♒	−0·18					
					July 28–Sept. 3	1 to 15	+36	$R_{1,2}$ (G.H.)	...	(0)	2·5	(A)	(a)	July 26–Au. 11, '73, 2+27, Den. July–Aug. '76, 6+27, Den. Aug. 2–11 '76, 10+42, Den. (B) (b)
					July 27, Aug. 23	7, 0	+32, 33	T. 45	...	18, 9	7·5, 6	B″′	B‴	[July 30, 345+40, S.Z. 135; 15ᵈ, 13°, C″C′. Aug. 2, 340+42, Hind, '75; 12ᵈ, 18°, c′C′. July 16–Aug. 16, 340+42, Den. '76; (0ᵈ), 19°, (?).]
					Aug. 8–13	2	+29	Denza (T. 45, 51, 62)	...	1	9·5	*b*	*b*	
					Aug. 1–31	11	+30	Schm.	...	(0)	11	(*b*)	(*b*)	
32	35	...	41	...	Aug. 16	89	+ 6	1808 II ♌	+0·07					
		23	...	...	Aug. 23	92·5	0	1797 ♌	−0·0)					
					Aug 29 '70	(78	+23)	T. 72	...	13	20	?″	?″	Subradiant (?) of a bright shower seen on Sept. 8–10, 1869 (T. 72).
					Aug. 31	85	−15	T. 67	...	8	17	*c′*	*c′*	
33	31	19	42	(38*a*)	Aug. 27	49	− 9	1596 ♌	−0·25					
33	...	20	...	(43)	Aug. 31	47·5	− 6	1845 III ♌	−0·36					

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33	...	24	...	(44)	Sept. 10	53	−16	1854 IV ☌ (W.)	+0·02				
				(39)	Aug.	53	+ 1	Schm.	...	(0)	10	(*b*)	(C)
					Aug. 20-25	53	+ 1	T. 65	...	2	11	*b*	C,
				(44)	Sept.	55	− 6	Schm.	...	(0)	8	(B)	(B)
					Sept. 3–27	66	−22	Schm.	...	(0)	15	(C)	
37	...	42*a*		(53)	Sept. 8	100	+59	1858 VI ☌ (Donati's)	−0·29	...	...	...	...
					Aug., Sep., Oct.	101	+57	D. T.S. &c.	...	(0)	2	(A)	...
					Sept. 1–15	99	+57	D. S.	...	(0)	2	(A)	...
37*a*	...	...	43	...	Sept. 20	44·5	−24	1763 ☌ (W.)	−0·03				
				(42)	Sept.	40	− 8	Schm.	...	(0)	16·5	(*c*)	(*c*?)
35	36	24	44	(42)	Sept. 26–27	62	−13	961 ☌ and ☊	−0·03 (−0·04)				
					Sept. 13–15	65	+ 6	T. 75	...	[0]	19+	[?]	?
					& sub-radt.	68	− 5	T. 76	...	[0]	10+	[*b*]	*b*
				(42)	Sept. 3–27	66	−22	Schm.	...	(0)	10	(*b*)	(B*a*)

Greg, 95 + 54, Aug.; and A.S.H. 87 + 50, Sept. 27; see also No. 53.

40 meteors, from Tupman, Lucas, Italian, and other observations.

33 meteors in Italian observations.

And Sept., 33−6, Schm., = (?),

Average of three close showers; pretty bright; 1869.

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 390.	Brit. Ass. Rep. 1875, p. 289.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☄'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
36	39	26	45	(55)	Sept. 19	17·5	+18	1769 ☋	+0·78					
					Sept. 28	24·5	+17·5	1769 ☋	(−0·02)					
					Sept. (1–10)	17	+ 9	Schm.	...	(9)	3	(A')	(A')	
						21	+18				11	(b')	(B')	
					Sept. 17–Oct. 21	15	+11	D. 1876	...	(0)	11	(b)	...	(*Piscids* I) at δ, η *Piscium* (see No. 55).
37	38	25	46	...	Sept. 19	145	+49·5	1683 ☋	+0·175					
					Sept. 30	172·5	+68	1840 III ☋	−0·15					
					Sept.	142	+67	Schm.	...	(0)	18	(c)	(c)	
											11	(b)	(b)	
38	40	27	47	(52)	Oct. 4	54	+52·5	1847 VI ☋	−0·265					
					Oct. 1–15	51	+61	A_{18} (Heis)	...	(0)	9	(B)	(B)	The last of Heis' *Perseo-Cassiopeiads*, A_{12-18}, Aug. 16–Oct. 31, all close to this.

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39	...	...	48	...	Oct. 9	112·5	− 7	1723 ♌	+0·065					
					Oct.	115	−10	Schm.	...	(o)	4	(*a*)	(*a*)	
					Oct. 11, 16	107, 1	− 2·5, 0	T. 87	....	2	7	B	B	Two close positions.
					Oct. 14	110	+ 6	T. 82	...	5	13	C,	(*c*)	
	...	...	49	...	Oct. 7	134	+77	1825 II ♓	−0·115					
					Oct. 1–15	105	+81	N_{1}, (Heis)	...	(o)	7	(B)	(B)	N_{13-18} (Sept. and Oct.) are all sensibly at this place.
					Sept. 20–Oct. 29	161	+84	D.	....	o	(8)	(B)	...	Observed 1876.
41	...	...	50	...	Oct. 16	61	− 7	1580 ♌	+0·18	...	...	...	...	Schm., 18–27 Oct., 87−2, is too distant; and ♆ (H.N.), Oct., 50−4, (*b*), which is compared with it by Schmidt, appears to be distinct,—(*c*) in list (*a*).
					Oct. 5, 6	54	−14	T. 78	...	10	8	B'	*a*'	
					Oct. 12, 13	76·5	−10	T. 76 (2 pos.)	...	3	17·5	*c*	C	
43	41	28	51	...	Oct. 19	39	−29·5	1779 ♌ (W.)	−0·02					
					Oct.	40	−30	Schm.	...	(o)	1	(A)	(A)	And Oct. 19–27, 34−14, (o^{d}), 17°, (*c*).

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 239.	This List.	Comp. No. in this List.	Shower date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☌'s Node (or Appulse): + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
42	...	...	52	...	Oct. 19	2	+54	1850 II ☌	−0·22					
					Oct. 22–28	5	+53	Schm.	...	(0)	2	(A)	A	Heis, B_{16}, and Schm. for end of Oct., at 337+57 (close average), (*Cc*).
				(47)	Oct.–Nov. 8	15	+52	D. (*Cassiopeiads*)	...	(0)	8	(B)	...	20 meteors, 1876, confirmed by reductions of Hung. obs. of 1872; Corder gives Oct., 22+58((0ᵈ),10°,(*b*)),'76.
41	42	29	53	(42*a*)	Oct. 21	81	+57	1842 II ☌	−0·14	...	...	...	...	[Dble. *Aurigid* ('76) rad. cont., Den., Nov. 22–Dec. 14, 80+42 & 80+50, } = F_3; *sees* No. 59].
					Oct. 25	78	+60	1848 I ☌	−0·23					
					Sept. 28	83	+54	S.Z. 153	..					
					Oct. 14–25	90	+58	D.	...	(0)	4·5	(*a*)	...	Obsd. 1876, *'Aurigids'*; { & Nov.–Dec. 8, 69+66. Schm., Nov., 66+65.
				(59)	Sept. 17–Nov. 24	83–92	+50–55	$F_{1,2}$ (G.H.)	...	(0)	7	(B)	(B)	Other showers referred to these comets in the R.A.S. list, 1876, are here assigned to another comet (*sees* No. 59).
					Oct. 15, 16	86	+45	T. 92	...	5	13	C,	C,	

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45	43	30	54	(61)	Oct. 22	157	+39	1739 ♓	+0·08	(*Leonid* sub-shower)		Schm., Oct., 140+23, (o^d), 20°, (?); and Corder, Nov. 9, 146+33, 17^d, 10°, *b*" (Den. Nov. 20–Dec. 13, 155+36, *see* below, No. 65).
					Oct. 3–20	142	+44	LG (G.H.)	...	(2)	11·5	(*b*)	(*c*)	
				(65)	Nov. 7	160	+40	T. 97 (estimd.)	...	15	2	A"	A"	
46	...	...	55	(45)	Oct. 8	19·5	+19	1757 ♉	(+0·08)	...	...	...	...	*Piscids*, renewed from No. 45, and continued in No. 68.
				(68)	Oct. 29	30	+26	„ ♓	−0·33					
					Oct. 17	24	+26·5	Gruber I	...	(o)	[4]	[*a*]	...	Gruber's Oct. Rads. V, VII, X, XIII, XIV are all near this comet-shower.
					Oct. 19–27	33	+21	Schm.	...	(o)	[6]	[*a*]	(*a*)	
					Nov. 3	30	+22	T. 94	...	5	4	*a*,	*a*,	
					Nov. 9–10	23	+10	C. 1876	...	...	...	...	...	(And 12+33) 5 ↓° each, Corder, '76. These and Heis P, Oct. 16–31, 23+40, (*c*) are doubtful accordances with the ♂ ♓.
47	...	...	56	...	Oct. 14	278	+53	1857 IV ♓	−0·26					
					Nov. 1	318	+53	1695 ♓	−0·12					
					Sept. 17–Oct. 25	317	+57	D. 1876	...	(8)	4	(*a'*)	...	Average of 2 feeble showers, 1876; and 10 *Cygnids* in Aust. Obs., Oct. 19, at 300+55.
					Nov. 1–13	282 307	+57 +53	Schm.	...	(18) (o)	4·5 6	(*a*;) (*a*)	... (*a*)	And Heis D, Nov. 1–15, 279+56.
					Nov. 7–25	299	+50	D. 1876	...	(6)	10	(*b*,)	...	Corder, Nov. 10, 334+40, not clear; and Nov. 16–17, 310+60, 2 or 3 ↓° 1876. (Continued in Jan. 1877 at 295+53, 20 ↓°, Denning.)

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Mon. Not. R.A.S., vol. xxxvi. p. 220	Reference Nos. Brit. Ass. Rep. 1874. p. 330.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☄'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement by Symbol used in This List.	Accuracy of Agreement by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°							
			57	...	Oct. 16	209·5	+42·5	1864 IV ☌ (W.)	+0·045					
					Nov. 1	205	+48	1097 ☌	−0·06					
					Nov. 13–Dec. 10	201	+44	D.W.S.	...	...	...	...	...	8 ↓[s] Aust. & Ital. Obs. 1872.
					Nov. 21–Dec. 20	208	+43	D.	...	...	...	...	...	17 ↓[s] observed 1876.
48	...	...	58	...	Nov. 4	104·5	+27	837 I ☌	+0·34					
					Oct. 20–26, 1839	99	+26	Herrick	...	9	5	a'		
					Oct. 22–27	109·5	+25·2	Gruber IX		8	5	a'		
					Oct. 21–25, max. 23 (A.M.), '68	111	+29	S.Z. 161 (a fine shower)	...	12	6	b'	B,	And T. 90, Oct. 12, 105+24, 23[d], 3°, =A;. S.Z. 169, 172, Nov. 12, 17, close together at 117·5+38·5, 10[d], 15°=c'; and T. 82, Nov. 7, 101+7 =? are far positions. *'Gemellids.'*
					Oct. 18–27	108	+12	Schm.	...	(8)	16	(d')	(d')	
					Nov.	113	+14			(0)	16·5	(c)	(c)	
					Oct. 25–Nov. 23	110	+23	P. 1876	...	...	...	...	...	12 ↓[s] *'Gemellids'*; distinct from *'Geminids,'* Nov. 21–Dec. 21, at 107+33.
					Nov. 16–17	106	+23	C. 1876	...	...	...	...	...	2 good ↓[s] and two or three others.

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49)	44	...	59	(53)	Nov. 9	89	+36	1582 (??) ♉	0·0(?)				
44					Oct. 16–31	72	+44	A_{16} Heis	...	(9)	14	(C')	(C)
					Oct. 19–Nov. 10	71	+43	D. 1876	...	(0)	16	(*c*)	...
44	...	...	...	...	Oct. 24	77	+45	S.Z. 162	...	16	12·5	C"	(B')
					Nov. 10	87	+47	S.Z. 166	...	1	11	*b*	
44	...	...	...	...	Oct. 10–27	71	+31	Schm.	...	(13)	16	(*c'*)	(*b'*)
					Nov.(10–14?)	82	+45		...	(0)	11·5	(*b*)	
					Nov. 7–17	75	+45	C. 1876	...	(0)	17	(*c*)	...
					Nov. 7–10	86	+36			0	2·5	A	...
49	44	...	60	...	Nov. 11	86	+19·5	1821 ♌	+0·03				
					Oct. 17–Nov. 13	90	+15	O (G.H.)	...	(0)	5·5	(*a*)	(*a*)
					Oct. 10–27	79	+13	Schm.	...	(15)	9	(*b''*)	...
					Oct. 18–27	93	+17			(15)	7	(B")	...
				(64)	Nov. 2?–Dec. 8	80	+23	D. 1876	...	11	6+	(B')	...

And Nov. 22–30, { 79 + 21, 5 meteors. 85 + 33, 5 meteors. } 1876, Den. *See* No. 60 (Den.); with separate show. Oct. 14–25, at 90 + 58 (No. 53).

Double *Aurigid* rad. Nov. 22–Dec. 14, at { 80 + 42 / 80 + 50 } D., 1876; *see* No. 53.

General rad., with a special position, at 86 + 36, of large meteors, on Nov. 7, 9, 10, 1876.

= T. 79 (Oct. 5–17); Gruber II, VIII, Oct. 17–24; S.Z. 156, 7?, 8, 160, Oct. 13–21; '*Orionids*,' max. Oct. 18–20.

Schm., Nov., 79 + 5, (o^d), 16°, (*c*); T. 99, Nov. 10–11, 77 + 10, 8^d, 11°, C' = C; S.Z. 165, Nov. 10, 70 + 20, 1^d, 14°·5, C = *c*.

20 ↓ᵉ confirmed by Greg; also by Aust. observations, Dec. 7–13, at 83 + 23; ('*Taurids II*,' *see* No. 64).

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Mon. Not. R.A.S., vol. xxxvi. p. 208	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☌'s Node (or Appulse); + without (or North of) ⊕'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement by Symbol used in This List.	Accuracy of Agreement by Symbol used in List (a)	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
	45	31	61	(54)	Nov. 13	150·5	+23·5	1866 I ☋	−0·015					
				(65)	Nov. 13–14, 1866–67	149	+23	*Omnes*	...	0	1·5	A	A$_e$	The '*Leonids*'; generally observed; chiefly on Nov. 15, A.M., after 1871.
					Nov. 19–20, 1876	149	+22	D. 1876	...	7	2	A′	...	5 ↓^{s}, evidently *Leonids*.
50	...	...	62	...	Nov. 24	147	0	1813 I ☋	−0·30					
					Nov. 25–Dec. 21	148	+ 2	D. 1876	...	(1)	2·5	(A)	...	11 ↓^{s}, principally on Nov. 26–29 (*Max.*) at 153−1.
				(71)	Oct. 31–Dec. 12	134	+ 6	LH (G.H.)	...	(0)	14·5	(C)	...	And Schm., Dec., 146+16, (7^d), 16° = (*c*′). *See* No. 71.
	46	32	63	...	Nov. 28, '52	23·4	+43	1852 III ☋ (W) (Biela's comet)	0·005					
					Nov. 27, '66	25·2	+42		(Hind)					
					Dec. 6, 1847	25	+40	Heis	...	8	3·5	*a*′	...	At Aix-la-Chapelle (*die Periodische Sternschnuppen*, 1849).
					Nov. 30, 1867	17	+48	S.Z. 176	...	3	6·5	B	...	9 ↓^{s} (Zezioli) with good radiant-point.
					Nov. 27, 1872	25	+43	*Omnes*	...	1	1·5	A.	...	The '*Andromedes*'; generally observed; and also (?) on Nov. 24, 1872.
					Nov. 16–17, 1876	24	+43	D.	...	...	...	...	...	6 ↓^{s}; indistinct shower.

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50*a*	...	...	64	...	Nov. 27	56	+20	1702 υ	−0·07	...	...	...	...	Schm. { Oct., 50+2. Oct. 10–22, 62+6. Den., Oct. 21–29, 1876, 61+18.
					Oct. 25–Nov. 21	64	+18	RG_2 (G.H.)	...	(8)	5·5	(*a'*)	(A; A"B')	The '*Taurids* I' of November; = T. 91, Nov. 7–12, 51+13·5, 56+21; = Heis, R_4, Nov. 14, 55+16; = D., Nov. 8, 58+16, and 20 (A.M.), 62+22·5 (and 65+12); = C., Nov. 16, 58+28, 63+9; and = Backh., Nov. 5, 54+16.
					(Nov. 21, '65	56	+13	Detonating fireball, Engl.						
					Nov. 23, '77	62	+21	Great ditto ditto)						
					Nov. 28–Dec. 24	57	+25	D. (*Taurids* III)	...	(1)	8	(B)	...	AG_1, Dec.–Feb., at same place as *Taurids* I = '*Taurids* III.'
				(60)	Nov. 10	70	+20	S.Z. 165	...	19	12·5	C;	...	(S.Z. 157, Oct. 21, 75+25, *see* 60.)
					Nov. 22–Dec. 14	79	+24	D., & C., 1876	...	(0)	22	?	...	'*Taurids* II.' A new shower, with max. on Dec. 6, 1876, at 80+23; Denning; *see* No. 60.
51	47	...	65	(54)	Dec. 2	162	+34·5	1798 II *m*	−0·14					
				(51)	Nov. 20–Dec. 13	155	+36	D. 1876	...	3	5·5	...	...	Chiefly Nov. 26–29, a good max. (21 ↓°, and 5 besides); Aust. reductions Nov. 13–Dec. 13, 160+32, 10 ↓°; continued in Ital. and other obs. in Jan., Feb., and March.
					Dec. 9	154	+26	S.Z. 182	...	7	10	*b'*	*b'*	S.Z. 177, 178, 186, *cd* C;, Nov. 30–Dec. 23, are also near. T. 97, Nov. 7, (B;), is far off in date.
					Dec. 5–14	163	+32	C. 1876	...	...	...	...	...	Doubtful (sub-rad. of *Ursa-Lyncids*).

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Mon. Not. R.A.S., vol. xxxvi. p. 220.	Reference Nos. Brit. Ass. Rep. 1874, p. 350.	Brit. Ass. Rep. 1875, p. 229.	This List.	Comp. No. in this List.	Shower-date, or Duration (1875).	Position of Radiant-point (1875). R.A.	Position of Radiant-point (1875). N. Decl.	Comet's Node, or Appulse (Ω). Authorities and Designations of the Meteor Showers.	Earth-distance of ☿'s Node (or Appulse); + without (or North of) ☿'s Orbit.	Discordance of Date and Position of Meteor Radiant-point in Days.	Discordance of Date and Position of Meteor Radiant-point in Degrees.	Accuracy of Agreement, by Symbol used in This List.	Accuracy of Agreement, by Symbol used in List (a).	Remarks, and Notes of more distant Agreements.
(a)	(b)	(c)	(d)	(e)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
						°	°				°			
			66	...	Dec. 3	359	+53	1818 I (?) ♈	−0·20					
					Nov.–Dec.	342	+62	D. 1876	...	(0)	12+	(C)	...	14 ↓ᵉ, confirmed by 5 Ital. & Aust. ↓ᵉ Dec. 7–13, at 333+59. '*Cepheïds*.'
52	49	...	67	...	Dec. 6	200	+68·5	1812 ♂ (Pons')	−0·23·	...	...	(B) *c*, ?"	(C) *c*, ?"	Schm., Nov., 180+65; S.Z. 183, 187, Dec. 12, 23, *c c*. KG (G.H.), Nov.–Dec., 160+71, = (C).
					Dec.–Jan.	209	+67	D. 1876–77	...	(0)	3½	(*a*)	...	19 ↓ᵉ; 9 Aust. ↓ᵉ, Dec. 7–13 = 200+67; and 21 Ital. ↓ᵉ, Jan. = 205+67.
					Nov. 25–Dec. 14	210	+67	C. 1876	...	0	4	(*a*)		
54	48	33	68	(55)	Nov. 13	21	+ 4	1743 I Ω	(−0·025)	(13)	8	(B″)	...	Schm., Oct., 27−1.
					Dec. 21	11	− 2·5	„ Ω	−0·14	(4)	10	(*b*,)	...	Den., Oct. 19–Nov. 9, 20+14, max. Oct. 21, feebly continued to Dec. 8, 1876.

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					(Feb. 7	353	− 8	„ Ω	+0·025)	(3)	6+	(B,)	...	Corder, Nov. 9–10, 23+10, 5 ↓°, 1876.
					Oct. 18–Nov. 10	23	+ 8	150 (Greg, 1875)	...	(3)	4·5	(*a*)	...	At *e Piscium*, 25+8, Oct. 30, 1872; T. W. Backhouse.
					Dec.	4	+ 4	Schmidt	...	...	...	(*b*)	(*b*)	
			69	...	Dec. 12–17	200·5	+ 4·5	1846 VII ♈	+0·09					
					Dec.–Jan.	207	+ 5	D. 1876	...	(0)	6·5	(B)	...	10 swift ↓°; feebly confirmed by Austrian observations.
53	49	...	70	...	Dec. 20	221	+77	1858 I = 1790 II (Mechain's) ♉	+0·075	(5)	19	?,	?,	Heis, N_{28}, Dec. 1–15, 123+78.
					Dec. or Jan.?	240	+70	D. 1877	...	(12)	8·5	(B')	...	12 small meteors; strongly confirmed by Italian observations.
55	50	...	71	(54)	Dec. 26	132	+21·5	1680 ♉ (W.)	−0·05	17	16	*c*"	*c*"	S.Z. 181, 135+37, Dec. 9.
										(0)	15·5	(*c*)	...	Schm., Dec., 146+16, and Dec.–Jan., 117+13 (two showers at 120+10, and 115+15).
										(0)	17	(*c*)	...	
				(62)	Dec. 21–Jan. 5	130	+20	D. 1876-7	...	(0)	2·5	(A)	...	Feeble shower at δ *Cancri*, confirmed by other observations.
					Dec.	130	+30	Schm.	...	(0)	9+	(*b*)	(*b*)	Corder, Dec. 1876, 131+34.
					Dec. 12. '67	136	+30	Masters	...	14	10	*b*"	*b*"	A well-marked shower.

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The April Lyrids, and contemporary Meteor Showers.

By W. F. Denning, Esq.

The return of these meteors was slightly observed this year on the evenings of April 20, 21, and 22, the two preceding nights having been overcast. On the 20th a watch was sustained between $8^h\ 45^m$ and $10^h\ 5^m$, with a very cloudy condition of sky, and only 7 meteors were seen, of which 3 were *Lyrids* and 3 belonged to a radiant near *ε Herculis*. On the 21st and 22nd the sky was very clear, and two hours' watching each night, between $10^h\ 30^m$ and $12^h\ 30^m$, revealed 25 (6 *Lyrids*) and 22 (3 *Lyrids*) meteors respectively. The motions were very slow; only 2 were as bright as first magnitude stars. The *Lyrids* were trained, and nearly all of them foreshortened close to a radiant, very exactly defined at 272°+32°. Of the other visible showers the most prominent were at 293°+43° (*δ Cygni*) and 263°+62° (*ξ Draconis*), and several additional ones in *Quadrans*, *Cygnus*, and *Corona* were very slightly seen.

My recent observations of this shower show the radiant to be almost identical with Comet I., 1861, with which a connection has been inferred. Former observations, including my own, place the centre a few degrees N.E. of the cometary radiant at 270°+32°, and the agreement of the two this year is an interesting point. In 1873–4 I had noticed the absolute inaction of this radiant on April 21, whereas this year it certainly continued on that and the following night. There were no traces of it on the 25th.

Whilst writing on the subject of the April shooting stars, I may mention that I lately undertook the careful examination of many thousands of paths (recorded by various observers in the morning hours of April 19–23), with the idea of finding the chief showers eastwards of the *Lyrids*, and situated in *Cygnus*, *Lyra*, *Vulpecula*, *Draco*, &c. I selected nearly 300 meteors with a length and direction of path conforming to such radiants, and having plotted them upon star-charts, the centres of 18 showers were found as under:—

Meteor-radiants eastward of the Lyrids, April 19–23.

Radiant ☌ ☌	No. of ↓	Radiant ☌ ☌	No. of ↓	Radiant ☌ ☌	No. of ↓
*280+58	22	*314+49	13	*321+31	8
*294+41	25	*311+37	15	*292+14	7
*287+22	20	308+31	7	*275+11	7
*316+61	16	307+40	8	302+18	6
*284+44	11	*309+68	15	*332+42	5
*312+22	13	296+53	9	281+28	5

* Exact, and probably certain.

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The majority of these are new showers. Amongst the catalogues which I consulted (chiefly Dr. Weiss's collection of Austrian observations, 1867–1874, which contain a very large number for April) there were many short paths and several stationary meteors. It is not surprising that these showers have nearly all of them escaped detection hitherto. They are samples of feeble, long enduring systems with a stationary radiant, and it must be remembered that reductions of this kind have never been undertaken before from such a mass of different materials, which in fact have only recently been published. The durations from April 19 to 23 are of course merely provisional, and I am convinced that a person would get the same radiants from meteors recorded during short periods before and after the dates mentioned. The first shower in the table is already well known as the *Draconids* (No. 64, at 281°+57°, of Mr. Greg's Catalogue, 1876). Of the remainder the next three showers, each giving a sharply defined active radiant, are the best, and appear to be new. I believe they may all be relied upon, and will be verified in time. The one at 294°+41° (slightly preceding δ *Cygni*) I have already confirmed from my own observations this year.

In April there are also many radiants (at present quite unknown) in *Perseus*, *Auriga*, *Cassiopeia*, and bordering constellations, but it is obviously desirable that, inasmuch as the subject is becoming very wide and confused, only the most active and certain showers should be mentioned. Amongst these are positions at 106°+46°, 91°+58°, 80°+74°, 53°+70°, 40°+73°, 130°+79°, indicated by about 10–15 meteors each. Strongly suspected radiants lie at δ *Cassiopeia*, η *Persei*, *p-q* and *c* (Bode) *Camelopardi*, β *Geminorum*, &c. This region needs more watching in April.

Ashley Down, Bristol,
May 8, 1878.

Observations of the Transit of Mercury, 1878, *May* 6, *made at the Royal Observatory, Greenwich.*

(*Communicated by the Astronomer Royal.*)

In addition to the instruments belonging to the Royal Observatory, the equatoreals and detached telescopes returned from the Transit of *Venus* expeditions were mounted in preparation for the Transit of *Mercury*. There were thus 14 instruments, of apertures ranging from 12·8 in. to 2·8 in., available, to each of which an observer was assigned; but the Sun was completely hidden by cloud throughout the whole transit, with the exception of a short break about 10^m after internal contact, and the following are the only observations which appear worth recording:—

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Great Equatoreal: Aperture 12·8 inches.
Observer: W. H. M. Christie.

The planet was only seen from about $3^h\ 25^m$ to $3^h\ 40^m$ G.M.T., more or less through cloud, though the definition at first was very fine. It was examined with five different powers.

No. 2,	Negative, with neutral tint wedge	126
No. 2,	Negative, with solar diagonal and neutral tint wedge	205
Supplementary A,	Negative, with polarising solar eye-piece (three reflexions) with and without neutral tint wedge	198
Supplementary A,	Negative, with solar diagonal and polarising solar eye-piece	320
No. III.,	Positive, with direct-vision polarising eye-piece with and without a diaphragm, giving a field of about three times diameter of *Mercury*	133

The solar diagonal was provided with a Barlow lens, so as to lengthen the focus.

1. A minute bright spot was seen with all these eye-pieces very near the centre of the planet's disk. It was slightly diffused, but with a brilliant starlike nucleus. As far as I could judge, it did not shift its relative position with the place of *Mercury* in the field of view, but I did not test this for extreme positions, being occupied with the scrutiny of the planet and its neighbourhood during the short time available. In the course of the observations, however, the position of the planet in the field of view was changed repeatedly. The bright spot was only seen when the eye-piece was carefully adjusted to focus, and the definition good as tested by the distinctness of the "rice grains" on the Sun. The spot was central, as nearly as I could estimate; if anything it appeared slightly on the side away from the point of contact, but I could not make sure of this.

2. A bright halo of somewhat irregular outline, and having a breadth of 3″ to 4′ was seen round the planet with an inner and much brighter ring about 1″ in breadth. The halo was much brighter than the ordinary surface of the Sun. It may have been an effect of contrast. It was best seen when the light was reduced to extreme faintness by turning the Nicol of the direct-vision solar eye-piece. The inner and brighter ring was only seen with this eye-piece. No effect on it or on the bright spot seemed to be produced by the diaphragm limiting the field of view to about 40″, but clouds obscured the Sun before I could do much with the diaphragm.

Simms' Equatoreal, No. II.: Aperture 6 inches.
Observer: E. Dunkin.

At the time of external and internal contacts the sky near the Sun was densely cloudy. A few minutes after internal contact,

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about 3^h 25^m M.T., a break in the clouds, which had been gradually coming up from the S.W., reached the Sun, and from that time till about 3^h 42^m M.T., *Mercury* was generally visible on the Sun, and occasionally seen free from cloud. A negative eye-piece, giving a power of 150, was used, with a solar diagonal and neutral tint wedge.

When first observed *Mercury* had advanced some distance from the Sun's limb, but was still partly obscured by clouds. These gradually cleared away, when both the Sun and planet were seen superbly defined; in fact, I can scarcely remember seeing a better definition of the granular surface of the Sun. *Mercury* was intensely black, except that I saw distinctly, for about a quarter of a minute, a very minute point of light near the centre, a little towards the following limb of the planet. There could, I think, be no doubt of this phenomenon, though the spot was not continuously visible during my examination. After an interval of two minutes the point of light became more vividly distinct than before. The passing clouds, becoming denser, prevented any further observations.

During the few minutes of superb definition, the ring, or corona of light, noticed around *Mercury* at the transit in 1868, was clearly visible, its breadth being about one-third that of the planet. To my eye, this corona was evidently fainter than the Sun, and appeared with radiating arms as in the solar corona. Looking at the intensely black disk of *Mercury*, in conjunction with the bright surface of the Sun, the halo of light seemed to be of a half-tint, and my impression at the time was that this ring of apparently subdued light was probably produced only by contrast. The halo was perfectly uniform around the planet. I had prepared to examine both the corona and the luminous point on *Mercury* with various powers, but before I could re-focus the instrument, using a higher power, the clouds became too dense to continue the observations.

Equatoreal by T. Cooke: Aperture 6 inches.
Observer: Captain G. L. Tupman.

After the ingress the Sun was almost, if not entirely, free from clouds for fifteen or twenty minutes, although never free from a thin white haze. The definition was not nearly so good as it had been during the week previous. There was a good deal of "boiling." I scrutinised the planet most diligently with powers of 110, 140, and 240, with and without the solar diagonal, using when necessary an achromatised wedge of bluish neutral tint glass. I could not detect the slightest trace of a permanent white spot within the disk. Occasionally there was a faint white diffused light over parts of the planet's disk, but it was not permanent, appeared in different parts, and sometimes on both sides of the centre.

I could not detect any ring, either luminous or shadowy,

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surrounding the planet. I was struck with the extreme sharpness of the planet's outline.

I was prepared to take measures with the double-image micrometer, but the planet was too far on when first seen to measure its position, and the clouds generally made such observations impossible.

Hodgson Refractor: Aperture 6 inches.
Observer: G. S. Criswick.

The Sun at the time of ingress was completely hidden by clouds, and did not become visible till the planet was well advanced on the Sun's disk. I then noticed that *Mercury* appeared to be surrounded by a ring of light equal in breadth to about two-thirds of the diameter of the planet, irregularly defined, rather fainter and whiter than the light of the Sun. As I continued to watch the planet, this ring seemed sensibly to contract in breadth, and to completely disappear as the image of the Sun became less bright from passing clouds. Two bright spots were also seen on *Mercury*, one, the fainter, nearly, but not quite, in the centre of the black disk, a little to the right or eastward; the other, and brighter, being nearer to the upper limb. These spots were not always visible to me, and I thought that they appeared sometimes slightly to shift their position with respect to the disk of the planet, but not *inter se*.

I am inclined to think that the ring of light seen round the planet was, as it appeared to me, merely the effect of contrast, as it was not visible during the later half of the time that the Sun was visible.

With regard to the two bright spots, except that they were certainly there, the shortness of the time available, and the more or less cloud about, prevented my having any opinion.

After the clouds had so obscured the Sun that it was impossible to say whether it was in the field or not, *Mercury* was most distinctly visible.

The telescope used was the "Hodgson," with a solar diagonal eye-piece, and a power of 180.

Sheepshanks' Equatoreal: 6·7 in. Aperture. Power 70, Negative Eye-piece. *Observer*: A. W. Downing.

The planet was not seen till quite on the Sun. An appearance of a ring slightly brighter than the Sun was visible round the planet, but no bright spot could be seen with this eye-piece.

Tried a negative eye-piece of power 140, and the existence of a spot was *suspected*, but the definition was not good with this power. The sky was more or less cloudy all the time, and a careful examination could not be made.

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Simms' Equatoreal, No. II., and Lee Equatoreal: Aperture 6 inches.
Observer: W. G. Thackeray.

Neither the Sun nor *Mercury* was visible, on account of the clouds, till about ten minutes after the time of ingress. On looking at *Mercury* through the telescope used by Mr. Dunkin it had a uniformly dark appearance, but after a minute or two I saw a confused patch of light in the middle of the planet, which appeared to vary in size and brightness according as the Sun was more or less obscured by clouds, and left me under the impression that the light was due to contrast. I afterwards looked through the telescope used by Captain Browne (Lee Equatoreal); *Mercury* then appeared of a uniformly dark colour, but by this time the clouds had become much thicker, and the Sun's limb was very faint. When the Sun was no longer visible *Mercury* could be seen in the telescope, and then the centre appeared the darkest.

Detached Telescope, Simms No. I.: Aperture 6 inches.
Observer: W. C. Nash.

No observations made at ingress. Sky cloudy.

About $3^h\ 28^m$ *Mercury* was seen on the Sun's disk, and for a time afterwards.

With telescope, Simms No. I., using powers 145 and 240, I carefully examined the planet, but could detect no signs of a ring surrounding it, or of a bright spot upon its disk.

Later on, I examined *Mercury* under favourable circumstances with the Lee Equatoreal, but could not detect a bright spot.

With the photoheliograph three photographs were obtained by Mr. Maunder on wet plates, which seem to show some traces of a bright halo round *Mercury*. A number of dry plates had been prepared by the tea process, but there was no opportunity for exposing them.

Royal Observatory, Greenwich,
1878, *May* 8.

Other Observations of the Transit of Mercury, May 6, 1878.

Mr. Brett writes:—

"A part of the transit was observed by me, and by Mr. Walter Pye, and by my assistant observer, at my Observatory near Putney.

"The contacts were hidden by an impenetrable mist.

"The planet was first seen after it had passed within the Sun's

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limb a distance nearly equal to its own diameter; and from that time until $3^h\ 45^m$ G.M.T. it was frequently seen at intervals through a film of cloud more or less dense, but which served very well the purpose of a dark glass.

"The planet appeared as a black disk surrounded by a ring of bright light. When the clouds thickened of course both were hidden, but the black disk never for a moment appeared without the ring of bright light.

"My impression at the first appearance was that the width of the ring was equal to a third of the diameter of the black disk. I immediately expressed that as my opinion, and it was echoed by Mr. Walter Pye, who was observing with another telescope, but he added that at the first appearance it was not round; and he afterwards explained that the bright ring was not concentric with the black disk; the ring being narrower on the side next the Sun's limb. When the next break in the clouds occurred this eccentricity was not seen. We continued to have good views of the planet and the ring during half an hour, definition being perfectly sharp; the cloud usually enabling us to dispense with the dark glass; and at all times when the disk of *Mercury* was visible the bright ring was conspicuous. My assistant observer, whose eye is both keen and well trained, confirmed me in this.

"We were obliged to use a very low power on account of the clouds. Mr. Pye had a power of 50 on my 3-inch Cooke, whilst I employed rather less on the 4-inch Cooke Equatoreal belonging to the Society."

Mr. G. F. Chambers, at his Observatory, Northfield, Eastbourne, Sussex, writes:—

"About half an hour before sunset the Sun appeared through the clouds for some ten minutes, and I was enabled to get a few glimpses of the planet. I could detect no indications of a spot, but a ring was visible round the planet. The breadth of it was slightly more than the diameter of the planet, but I had no chance of making any measures. My wife, whose vision is very acute, described it as a dark ring, but I am unable to feel quite sure whether it ought to be described as bright or dark.

"I will only add that I used a refracting telescope of 4 inches aperture and a power of 60."

Mr. Cole writes:—

"I send the following notes (for what they are worth) of the ingress, as seen at Sutton (Surrey) by myself and others.

		External Contact. h m s	Internal Contact. h m s
J. J. Cole.	Refractor: Aperture $6\frac{1}{4}''$, reduced to $2''$; diagonal eye-piece.	3 12 0 (estimated from half on limb $13^m\ 15^s$).	3 14 30

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		External Contact. h m s	Internal Contact. h m s
R. L. Cole.	Refractor: Aperture 3¼ in., reduced to 1¼″; image on screen.	3 12 9	3 14 30
A. W. Bawtree (½ of mile away).	Refractor: Full aperture 3″; image on screen.	3 12 18	3 14 30

"The Sun became nearly clear at $3^h\ 5^m$, and remained so without obvious cloud until $3^h\ 25^m$. After that the planet was not seen on the disk.

"The definition during ingress was very good. There was not any black drop or 'stickiness' between the limb and the planet that I myself could detect. Our time was probably correct to a few seconds."

Mr. Penrose, at Colebyfield, Wimbledon, writes:—

"The sky was very thick until a few minutes before the predicted ingress. Then there seemed some hope of an opening, and a 'ghost' of the Sun appeared. Fearing, however, want of distinctness if I used the reflecting solar eye-piece which I had prepared, I changed it for a direct positive eye-piece magnifying 75. Focus rather roughly got; still, I had a tolerable definition. The sky cleared very much, and towards the time of internal contact the Sun's heat slightly scarred the grey glass screen I was using. The time observations are given on another page. After these were taken I replaced the solar eye-piece, using a magnifying power of 110; and both I and another observer saw a flickering spot of light on the planet's disk. To me it appeared about one-fifth of the planet's diameter above the centre, that is, in the direction of the Sun's centre. I consider it to be an optical effect entirely.

"The definition was good until about 15 min. after ingress, after which nothing could be observed.

	° ′ ″	
Latitude	51 24 57	N.
	h m s	
Longitude	0 0 55·2	W.

"Tube and object-glass by Steinheil. Mounting and clock by Troughton and Simms.

Aperture	5¼ in.
Focal length	83 in.

"Time by a watch compared before and after the observations with a sidereal clock by Aldridge; rate, +0ˢ·5 per diem, as ascertained by good meridian observations of stars on the evening of May 4, and of ☿ on the mornings of the 5th and 7th, civil reckoning. Two assistants were engaged in recording the time, one watching the minutes and seconds, the other writing down the times called for.

"The times here given are reduced G.M.T.

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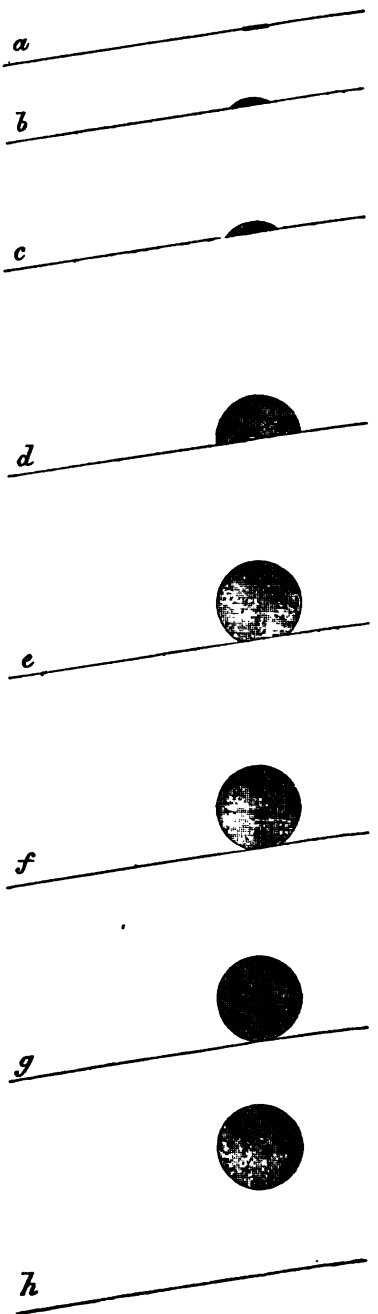
May 6, 1878, 3^h +

m	s		
11	3·5	*a*	Suspected dark bar on ⊙'s limb very uncertain; scud ensued for a short time; the trace then lost.
12	4·5	*b*	Strongly defined notch on ⊙'s limb.
12	13·5	*c*	Notch seemed to have doubled its area.
12	49·5	*d*	Apparent bisection of planet.
14	1·5	*e*	The area of the excluded segment seemed equal to that of the notch first seen.
14	31·5	*f*	Planet apparently circular, but clinging to limb.
14	35·5	*g*	Two thin threads of light seemed to stream between ☿ and ⊙'s limb.
17	54·5	*h*	Estimated distance from limb = diameter of planet.



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"From phases *b*, *c*, and *e*, I should deduce external contact at about $3^h\ 11^m{\cdot}35$. The phase *d*, making allowance for irradiation, is not inconsistent with this deduction."

Mr. Prince, at his Observatory, Crowborough, Tunbridge Wells, writes:—

"The first gleams of sunshine, since the early morning, occurred at $3^h\ 5^m$, $3^h\ 8^m$, and $3^h\ 10^m$ respectively. At the latter time external contact had *not* happened, and it was most unfortunate that at this moment heavy clouds came up from S.E. and totally obscured the Sun till $5^h\ 49^m$; but from this time till $6^h\ 25^m$ I had the opportunity, during intervals of a few seconds' duration, of noting down the following observations:—

"In the first place I found the atmosphere in such a disturbed and tremulous condition that any micrometrical measures were out of the question. The limb of the planet frequently resembled a slowly revolving cogged wheel. During moments of best definition, with a power of 90 within a Dawes eye-piece, having a half-inch diaphragm and a tinted wedge, a diffused greyish patch was visible *about* the centre of the planet (it was absolutely impossible to estimate its position more precisely, on account of the great atmospheric tremor); but upon changing the power to one of 250 the patch was no longer visible, and the planetary disk was uniformly dark throughout; upon returning, however, to the power of 90 it was again visible. An aureola of light, a little brighter than that of the Sun's disk, surrounded the planet. Its breadth varied from about six seconds when the Sun was partially obscured by drifting scud to about four seconds when it was free from cloud. The periphery of this ring was not well defined; but this, again, might have been due to atmospheric tremor. It had not the slightest trace of colour. My equatoreal telescope, late Dr. Pearson's, and made by the elder Tully (see *Memoirs of Royal Astronomical Society*, Vol. ii., p. 507), is of 6·8 inches aperture and 12 feet focal length. Latitude 51° 3′ 14″ north; longitude (in time) 38 seconds east."

Prof. Pritchard, at the University Observatory, Oxford, writes:—

"The observations of this phenomenon at Oxford will, I fear, only add another instance to the list of failures in the South of England. A battery of telescopes had been prepared, including a Sun spectroscope attached to the large Grubb refractor, for the purpose of examining the progress of the appulse across the chromosphere; but dense clouds proved fatal to success so far as observation of the ingress was concerned.

"However, about 19 minutes after the commencement of the transit, a few short and partial breaks in the clouds occurred, and during three very short intervals *Mercury* was steadily viewed in the De la Rue reflector of 13 inches aperture. The metallic plane speculum had been removed from the instrument, and its

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place supplied by a first surface reflection from a large Herschelian prism. A Dawes eye-piece was attached, with a diaphragm of ·3 inch, power 120, and the result was a calm and beautifully defined image of the planet on the Sun. During two of the glimpses, I entertained no doubt that the centre of the planetary disk was very decidedly brighter than the rest of the planet; but during the third and briefest glimpse I failed to detect the phenomenon. There was no aureola discernible round the limb of the planet. The appearance of conspicuous brightness near the centre was confirmed by Mr. Jenkins, one of our very intelligent assistants; but nevertheless, owing to the brief intervals during which the Sun was even partially unobscured, and the consequent impossibility of making further experiments, I should hesitate to affirm that the bright spot in question originated in actual physical conditions of the planet itself. I may add that the general appearance of the planet on the Sun's disk was by no means so decidedly black as I had anticipated, a remark which also occurred independently to Mr. Jenkins.

"*May* 8, 1878."

Mr. Proctor, observing at Clapham, writes:—

"The weather was altogether unfavourable for observations intended to assist in the interpretation of optical or physical phenomena presented by *Mercury*. About half an hour before ingress the sky was altogether clouded and rain was falling. Shortly after, the Sun could be occasionally seen through haze between the heavier clouds. For a second or two during egress a view was obtained of *Mercury*, rather more than half immersed, but the glimpse was too brief for any peculiarity of figure to be recognised, supposing any then existed. About $3^h\ 21^m$ the sky cleared about the Sun's place, but the aspect of the north-eastern sky showed that the opportunities for observing *Mercury* would be brief and imperfect. At this time I was using a Kellner, power about 120, with the two-prism solar eye-piece and Barlow lens. Later I used, for a very short time, a Browning C eye-piece with the same solar eye-piece. Throughout I used the full aperture (8½ inches) of my Browning-With reflector. At first the weakest darkening glass was required, but later the haze thickened so much that no darkening glass was wanted to protect the eye. During the few moments, however, in which I used the C eye-piece the neutral tint glass was added. Despite considerable motion in the upper air, and apart, of course, from transient effects of haze &c., definition was excellent.

"*Mercury* appeared as a uniformly dark disk, except near the centre, where there was a whitish spot of sensible magnitude, and brightest in the middle. It seemed very nearly as bright there as the general surface of the Sun; but I have no doubt that its apparent brightness was in part due to contrast with the blackness of the disk of *Mercury*.

"I now paid special attention to the bright ring; but I may

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as well complete the account of all I was able to notice about the bright point.

"First, it was quite as distinct when the solar image faded in lustre as when that image was brightest. But when any of the small scudding clouds, while darkening the greater part of the solar disk, left the image of *Mercury* unaltered, the spot grew perceptibly fainter. I was not looking for this, and attach therefore the more importance to it. The first time it happened I thought I must have been mistaken; but I afterwards noticed it unmistakably three several times. As the spot recovered its brightness it seemed three-cornered. But the change was too rapid for me to be certain about this; and when the Sun's image was steadily seen no triangularity could be recognised in the spot.

"I could not detect any displacement of the spot from the centre of *Mercury's* disk with the low power employed. Later, when I used the C eye-piece, I only had time to notice that the spot was very much fainter than with the Kellner. I was sliding out the neutral tint graduated glass to observe the spot better, when clouds covered the Sun, which was only seen during two momentary glimpses thereafter.

"I have no doubt whatever that the spot is a merely optical phenomenon. I may note that I could perceive no change in the appearance of the spot when the telescope was released from the driving clock, and *Mercury* allowed to approach the edge of the field.

"Around *Mercury* there seemed to be a ring brighter than the Sun's surface. But I am inclined to suspect that the completeness of this ring is an optical illusion. When I directed my attention to the side of *Mercury* remotest from the Sun's limb, I could not detect any trace of a bright ring, though I could see with perfect distinctness the darkening towards the well-defined limb of the planet. On the other hand, when my attention was directed to the side of *Mercury* nearest to the Sun's limb, the bright ring was very obvious and appeared to surround the whole disk. I feel satisfied that if the half of *Mercury's* disk nearest to the edge had been kept out of view, no bright arc would have been seen round the remaining half, but I had no time to try the experiment. I could detect no trace of colour in the dark part of the ring next to the planet's disk. But the day was very unfavourable for detecting colour. The disk of *Mercury* itself did not appear black, but only of a very dark grey, owing to the bright haze over the Sun.

"The bright ring seemed very fine, a mere thread; but its distance from the limb of *Mercury* was equal to nearly or quite one-third of the planet's diameter.

"Beyond noting that the ring could be well seen with the C eye-piece—more distinctly, in fact, than with the Kellner—I could make no observations on it with the higher power. For I had barely changed powers when the Sun disappeared—about $3^h\ 50^m$.

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Capt. Noble communicates the observations made at Antwerp by the Baron von Ertbon, and MM. de Boë and Schleusner.

The weather was favourable, and at the time of interior contact, the limb of the Sun was of an extreme sharpness such as is very rarely seen.

The interior contact was observed by

	h	m	s		h	m	s	
M. de Boë at	3	31	53·5 ⎫ M. T. Observatory of	= 3	14	14·9	G M.T.
M. von Ertbon	3	31	54·5 ⎭ M. de Boë	= 3	14	15·9	

No black drop nor ligament was seen: the limb of the Sun reappeared without any deformation.

The observation was thus exceptionally good. M. Schleusner was the first to recognise that a brighter aureola surrounded the planet; and that there was a bright spot on the surface of the planet. The breadth of the aureola was about equal to the radius; the bright spot had the aspect of the Sun's surface, as if this had been seen through a hole. These observations were confirmed by all the observers, with the different instruments.

β Leonis and B.A.C. 3992. By S. W. Burnham, Esq.

Admiral Smyth, in the *Cycle of Celestial Objects*, has noted a very distant companion to *β Leonis*, giving

$$P = 114 \qquad D = 298''.$$

No star was seen in this place by Powell; but in the opposite quadrant, about the same distance, he found a star of nearly the same magnitude, which he supposed was the one seen by Smyth, there being an error of 180° in the angle given above. This view is supported by the remark in the *Cycle* concerning a 7 magnitude star in the same direction at a greater distance. There is no bright star anywhere near *β* except the one in the *sp* quadrant, which is B.A.C. 3992.

Mr. George Knott, in his recently published work, gives measures of several stars in the field, including the two above referred to, and a very faint one near the place of Smyth. It is not probable that Smyth referred to this star. It is too minute, and does not answer the description in other respects. Errors of this kind are so common in the *Cycle*, and the stars themselves so unimportant, that it is hardly worth while to follow them up. Mr. Knott's measures of three stars in the field are as follows:—

	P	D	
	°	″	
A and B	206·4	282·42	1864·40.
A and E	115·4	303·50	1864·38.
A and D	201·5	1134·3	1864·37.

The last is the 7 magnitude star referred to by Smyth. Looking at *β* with the 18½-inch refractor of the Dearborn Observatory, I noted a star much nearer than any hitherto seen,

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but still too distant to be of any importance, unless as a light test. By a single set of measures I found

P = 345°0 D = 76″59 1878·21.

This star is excessively minute, and probably an aperture of at least 12 inches would be required to show it satisfactorily.

The distant large star (D of Mr. Knott's measures) I found to be a fine pair, moderately close, and considerably unequal. From two measures I found

P = 337°7 D = 1″52 1878·21 Mags. 6·7 . . . 11·0.

This is a very easy object with the large aperture.

A much more difficult pair was discovered, $2^m\ 17^s\ p$, and $35'\ n$ of *β Leonis*. The components are close and very unequal. One set of measures gave:

P = 73°4 D = 0″57 1878·15.

This star is Lalande 22262, 7½ magnitude, but more like 8 mag. to me.

Chicago, April 25.

Observations of Polar and Equatoreal Diameter of Mars, near Opposition 1877, *with Equatoreal Instrument*, 8-*in. aperture*, 110 *in. focal length.* By R. L. J. Ellery, Esq.

1877.	No. of Measures.	Eq. Diam.	P. Diam.	Micrometer.
		″	″	
27 August	8	24·550	24·185	Double-image micrometer; power 2.
29 August	10	25·488	24·918	D.-I. microm.; power 2.
30 August	20	25·082	25·172	D.-I. microm.; power 3.
6 September	10	26·806	26·171	Parallel-wire micrometer; Barlow lens, power 262.
	20	25·287	25·602	D.-I. microm.; power 3.
	10	25·268	24·747	D.-I. microm.; power 2.
16 September	12	24·137	24·484	D.-I. microm.; power 3.
17 September	20	24·046	23·688	D.-I. microm.; power 2.
22 September	20	22·772	22·947	D.-I. microm.; power 2.
26 September	10	22·248	22·230	D.-I. microm.; power 2.

N.B. The Airy double-image micrometer is supplied with four power lenses, No. 1 being the highest; the *power*, therefore, as spoken of above must be understood to be the power lens used, not the magnifying power.

The measures are not corrected for defect of illumination.

Observatory, Melbourne,
February 21, 1878.

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On a Solar Photograph. By Dr. L. M. Rutherford.

Dr. Rutherford writes:—

"Permit me to present to the Society a copy on glass taken from a negative of the Sun made at my Observatory on August 11, 1871. It represents a small portion of the Sun's disk with one spot, and a group of smaller ones; but it is not to these features which I desire to draw attention. Shortly before leaving America I saw a copy of the magnificent photograph of the Sun made by M. Janssen and read his communication on the subject in the *Comptes Rendus*. In this he says that his are the first and only photographs which represent the granulations of the Sun's surface. I do not claim that my photograph of 1871 is at all equal to that of M. Janssen; but I submit it simply as a correction to the history of this part of astronomy. It will be seen on inspection with a proper lens that the granulations, rice grains, or willow leaves are quite fairly visible on the copy presented.

"It was made with my 13-inch achromatic corrected for photography by the attachment of a correcting meniscus of flint glass, and with a very short exposure, the duration of which I have never measured.

"*London, May* 10, 1878."

Notes of some Errata in the Report of the Progress of Meteoric Astronomy during the year 1877. By Professor A. S. Herschel.

A few typographical and other errors present themselves in the "Report of the Council on the Progress of Meteoric Astronomy during the year 1877" (in this volume, p. 216, *et seq.*), the following proper corrections of which I am able, by some recently received information, to supply.

P. 229, in the table, column end-height, of the detonating fireball seen in the United States on Nov. 20, 1877, for "a little S.E. of," read "a little S. of E."

P. 231, line 24 from bottom of the page, for "Aug. 16," read "Aug. 17."

Professor D. Kirkwood informs me that the fall of an aërolite stated (p. 219, in the note) to have occurred in Georgia, U.S., on Jan. 20, 1877, proved on investigations which he made, to be a newspaper invention, no such occurrence as that of an aërolitic fall on the asserted date having really taken place.

The radiant point near *a Ceti*, noticed (p. 226) as having been observed by Mr. E. F. Sawyer, at Boston, U.S., on October 17, 18, and 20, 1876, Mr. Denning regards as being very imperfectly determined (see *Astronomical Register* for 1878, p. 106), as in a tracing of them with which he was furnished by Mr. Sawyer the courses of the meteors seen began near that star, and were for a position so near the radiant point too long to be probably conformable to the centre of divergence there which was assigned to them.

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. *June* 14, 1878. No. 8.

Prof. A. CAYLEY, F.R.S., Vice-President, in the Chair.

M. Edouard G. Deville, Quebec; and
John Frederic Main, Esq., B.A., University College, Bristol;

were balloted for and duly elected Fellows of the Society.

On the late Opposition of Mars. By Maxwell Hall, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

The contrast of the style of work to be performed in Transits and Oppositions is so strongly marked by my small telescope that for the future I shall pay attention to Oppositions alone, although Jamaica is well situated for some of the phases of the next transit of *Venus*.

You may perhaps remember that the observations made here during the last Opposition of *Mars* were so completely under control that they tested the going of the clock for very short intervals of time, and plainly showed that a good clock and a firmly mounted Equatoreal would give the solar parallax with great accuracy, in accordance with your article written in the year 1857.

I am awaiting the results of Mr. Gill's observations made with a Heliometer in the Island of Ascension; it would seem that they require some time for their reduction; and the system of triangulation of the comparison stars is certainly open to the objection that half the time and labour must be expended on the stars themselves instead of *Mars*, unless the triangulation is performed about midnight, when the circumstances of refraction will be different. In my extension of your method, by comparing *Mars* with two stars in the same sweep, one preceding and the other following, the work of connecting the stars together for

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controlling the reductions is made a part of the observations of *Mars* for the determination of the parallax.

The Observatory, Jamaica,
1878, *May* 7.

Note on the Great Comet of 1861. By J. Tebbutt, Esq.

In many of the recent popular works on astronomy I have noticed that frequent references are made to the fact that European astronomers have shown that the Earth passed through, or very near, the tail of the great comet of 1861. As I was not in communication with the European scientific journals at the time of this comet's appearance, my discovery of it was not announced directly to them, nor were the results of my observations and calculations made known in the same way. I wish therefore to put on record as briefly as possible, in the *Monthly Notices*, that I claim to be not only the first observer of the comet, but also the first calculator of the elements of its orbit. Having assured myself by means of the imperfect instruments at my command that the object discovered on May 13, 1861, was really a comet, for it moved extremely slowly for some days, I announced the discovery to the Sydney Observatory on May 21, and in the *Sydney Morning Herald* of May 25. My approximate determination of the orbit from an observation at the Sydney Observatory, on May 24, and sextant observations by myself at Windsor, on June 3 and 11, appeared as follows in the *Herald* of June 15:—

Perihelion passage	June $13^d.7253$ G.M.T.
Perihelion distance	0·82033
Longitude of ascending node	280° 0′ 44″
Longitude of perihelion on the orbit	252 13 39
Inclination of the orbit	86 18 42
Heliocentric motion	Direct.

In the same communication I pointed out the near approach of the Earth to the comet's tail for June 29, and also that the comet itself would probably become visible in full daylight. This announcement was followed by a more complete prediction by me in the *Sydney Empire* of June 22. The next published elements are those of the Rev. W. Scott, Government Astronomer, and Mr. H. S. Hawkins, M.A., of Goulburn, which appeared respectively in the *Herald* of June 27 and July 1.

As little seems to be known in the northern hemisphere respecting the early history of this comet, and as the newspapers which treat of it are of an ephemeral character, I have communicated this Note to the Society.

Windsor, N. S. Wales,
April 22, 1878.

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Note on the Belgian Observations of the late Transit of Mercury. By Captain W. Noble.

In reply to a question addressed to me by our President, at the meeting of the Society on the evening of May 10, I expressed my belief that the determinations of the geographical position of the Observatory of M. de Boë at Antwerp, and of the local time there, were entirely trustworthy. I am now in a position to speak even more definitely, inasmuch as I have received a letter from the Baron von Ertbon, in which he says (I translate literally from the original) "that he (M. de Boë) has connected his Observatory by triangulation with the tower of Antwerp Cathedral, which is a point of the first order in the triangulation of the country. The co-ordinates then by reference to Brussels and Greenwich are rigorously known.

	m	s				
Brussels is East of Greenwich	17	28·9				
From Brussels to M. de Boë's Observatory, East		9·7				
	17	38·6	Lat.	51°	12′	28″.

The time was rigorously known. M. de Boë has a meridian circle by Secretan, with an objective of 2·56 inches aperture, a clock by Garnier, and a chronometer by the same artist, of which the error and rate are regularly observed and noted."

The Transit of Mercury, May 6, 1878. By John J. Plummer, M.A.

The transit of *Mercury* was observed at Orwell Park under unfavourable circumstances, but a better observation was secured than could have been expected. About noon light cirrus cloud overspread the sky, which became gradually denser until towards sunset, when rain fell heavily. For the few minutes during which the contacts were to be observed the clouds were decidedly thinner and the Sun could be well seen through a slightly tinted glass. The definition of the limb was, however, very bad and unsuited for scrutinising the phenomena of contact, and, although I was prepared to take micrometrical measurements, none were attempted in consequence.

	h	m	s
Greenwich mean time of external contact	3	11	34·2
„ „ internal „	3	14	19·7

The external contact was considered very good, but it is possible it may be 3 or 4 seconds late, as the undulations of the limb were so considerable that a little hesitation could hardly be avoided. The time of internal contact was the moment at

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which light appeared permanently established in the rear of the planet without any distortion of the Sun's limb. Owing to the violent motion, it is possible that this time is one or two seconds too soon, but it is to be noted that my observation of the transit in 1868 left a precisely analogous impression on my mind at the time, namely that there was the reverse of clinging, or that the planet disengaged itself from the limb more suddenly and rather earlier than the progress of the phenomenon led the observer to anticipate. So far as the unfavourable atmospheric circumstances would allow, I note, therefore, no drop or ligament, no bright spot on *Mercury*, no aureola around the planet, and no distortion of either Sun or planet other than as produced by our atmosphere.

At $3^h\ 9^m\ 20^s \pm$ my wife, who assisted me at the observation, first detected the planet against the solar corona, though I had previously looked for it in vain. On taking my station at the telescope a minute later I also detected it, dimly visible, but sufficiently so to direct my attention to the exact point where contact was to take place, and to lead me to suppose that, but for the clouds through which the observation was made, it would have been fairly conspicuous. Full aperture employed 10 inches, focal length 12 ft. 9 in., power 303.

Orwell Park Observatory,
May 31, 1878.

Observations of the Transit of Mercury, May 6, 1878, *at Dunecht Observatory.* By Lord Lindsay and Dr. Copeland.

Until about half-an-hour before the beginning of the transit the day was very fine, with a fresh breeze from the S.S.E. About this time light drifting clouds began to appear, and increased in density until the Sun was at times wholly obscured. Just before first contact, however, the clouds gradually assumed a more fleecy character, and the Sun reappeared with rapidly increasing brightness. Thus it happened that first contact was seen through an appreciable layer of cloud, which was nearly swept away before the planet had fully entered on the Sun's disk. Except perhaps for photographic purposes, the sky was practically clear for the rest of the day.

Five telescopes were used in the observations, besides photographic apparatus. Lord Lindsay and Mr. Ranyard observed with the spectroscope; Mr. Ranyard also made eye observations along with Messrs. H. J. Carpenter, J. G. Lohse, and Dr. Copeland, while Mr. Davis took a series of photographs.

The observations are here given under five heads, in the succession adopted in Lord Lindsay's circular of April 16.

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Section 1.—Eye Observations of 1st and 2nd Contacts.

Mr. Ranyard at the time of first external contact was employed with the spectroscope. His time of internal geometrical contact, recorded by an assistant, is $6^h\ 2^m\ 15^s{\cdot}5$. Mr. Ranyard remarks: "At the time I called the word *contact*, it seemed to me that the two disks (i.e. the disk of *Mercury* and the Sun's disk) were in geometrical contact. I did not see the bridge of light separating the planet from the limb till about another second had elapsed." The instrument used was the 15·06 inch Grubb refractor, reduced to 10 inches, carrying a power of 380 on a Merz polarizing eye-piece.

Mr. H. J. Carpenter observed with a Newtonian unsilvered glass reflector of 12·9-inches aperture, having a total reflecting prism for the 2nd reflexion. Power used throughout 171. His notes are:—

"Owing to cloud, the first glimpse I got of the planet on the Sun's limb was at $5^h\ 59^m\ 30^s{\cdot}5$. By estimating the time when *Mercury* had encroached on the disk as much again as when first seen, I put the time of first external contact at $5^h\ 59^m\ 14^s{\cdot}5$ sidereal time. At $6^h\ 2^m\ 14^s{\cdot}4$ *Mercury* was fully on the Sun's disk. The time given is that when a thread of light seemed to come between the Sun's limb and *Mercury*; this phenomenon was almost instantaneous. The images were very bad, and I could not say that I detected any ligament or any alteration of the circular form of *Mercury*. I could not see *Mercury* outlined against the sky, nor detect a halo round it."

Mr. J. G. Lohse, using a 4-inch by Cooke, with a power of 60, gives "First external contact $5^h\ 59^m\ 11^s{\cdot}8$, probably one second late. Internal contact certainly past at $6^h\ 2^m\ 17^s{\cdot}3$; time given probably too late by 15^s to 20^s. Definition of limbs very bad at internal contact. The observation difficult, on account of a very obvious ligament. In accordance with the foregoing, I put

	h	m	s
External geometrical contact	5	59	10·8
Internal „ „	6	1	59·8

Dr. Copeland, observing with a 6-inch telescope, object-glass by Simms, mounting by Dallmeyer, and a power of 126, recorded—

	h	m	s
Just perceptible indentation	5	59	12·8
Apparent geometrical contact	6	2	7·3
Distinct and instantaneous rupture of ligament	6	2	19·3

After internal geometrical contact I was surprised to see a distinct clinging between the limb of *Mercury* and that of the Sun, thus forming a distinct narrow ligament. This ligament broke instantaneously at the moment given. The planet was

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then seen at an appreciable distance from the Sun's limb. The ligament was merely a faint grey shading, which might easily have been overlooked. It totally disappeared on breaking.

Section 2.—Spectroscopic Observations of Contact.

Lord Lindsay used a 6-inch Equatoreal by Cooke, within the focus of which was a direct-vision prism.* This threw a dispersed image of the Sun on the slit of an automatic spectroscope, by Browning, capable of giving an effect of from 2 to 10 prisms. The distance between the slit and the direct-vision prism in the tube was 13·1 inches. The length of the direct-vision prism is $3\frac{1}{2}$ inches. These dimensions are measured along the axis of the tube. The slit and the refracting angle of the direct-vision prism being set perpendicular to the Sun's limb, an arc of the Sun's limb depending in length on the power used is seen extending along the spectrum, and in fact tinted with the corresponding colours of the spectrum. In addition the bright lines of the chromosphere are seen projecting beyond the Sun's limb. By moving the telescope any part of the Sun's limb may be made to appear in any colour that may be wished. In this way the point where *Mercury* was expected was brought into the region of the C line.

Immediately before first contact the apparatus was further arranged as follows:—

Light passing twice through two half prisms of the Browning spectroscope, equivalent deviation 103° for line D, dispersion 8° 7′·2 from C to *h*. Magnifying power of eye-piece on viewing telescope 3, giving an effect of 45 on the whole instrument. The recorded observations are

h	m	s	
5	58	13	Sun's limb barely visible.
5	58	46·4	First glimpse of the planet on the reversed C line.
5	59	0·4	External contact on the C line.

The increasing brightness of the Sun rendering an addition to the dispersive power of the spectroscope imperatively necessary, Lord Lindsay made the requisite changes; when this was done it was unfortunately "found that the second contact was lost."

Mr. Ranyard had attached to one of the finders (aperture 3·74 inches) of the 15-inch refractor a Grubb compound prism spectroscope, dispersion C to *h* 5° 40′, magnifying power 55. With this instrument Mr. Ranyard, looking for *Mercury* outside the Sun's limb in the red end of the spectrum, could detect no indications of the presence of the planet off the Sun's disk.

* Dispersion from C to *h*, 10° 58′·4. In what follows, the magnifying powers and instrumental constants are substituted for the numbers or letters actually recorded by Lord Lindsay; in other respects his notes are strictly reproduced.—R. C.

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Section 3.—*Observations of Cusps.*

No measures of cusps were taken. All the observers agree that no brush or streak of light extending round or partly round the planet was visible during Egress. Except in the case of Lord Lindsay's observations, no indication of the planet was seen outside the limb of the Sun.

Section 4.—*Observations of the Planet when on the Disk of the Sun.*

After an internal contact, the sky becoming still clearer, Lord Lindsay changed the spectroscope to an effective dispersion of 6 prisms, giving 24° 21′·6 from C to *h*, the magnifying power being at the same time increased to 57. $6^h\ 4^m$ and $6^h\ 11^m$ the planet was viewed in the colour of the E line, with the note, "Disk of planet round, sharp, clear, clean."

At $6^h\ 20^m$, with the same arrangements, Lord Lindsay adds: "I see no sort of halo; haze very thick; wide slit." The greatest opening possible is 0·032 inch. Lord Lindsay's further observations are:—

At $7^h\ 7^m$. "Still with 6 prisms but with a power of 136; with this higher power there is a distinct decrease of darkness from the centre of *Mercury* outwards."

At $7^h\ 21^m$. "As before, but viewed on the C line, the shading off from the centre is most marked, with decreased aperture, and shows best with 1·63-inch aperture, mean of 6 measures with the Iris diaphragm."

$7^h\ 50^m$ to $7^h\ 55^m$. "Spectroscope removed; with Browning achromatic eye-piece, power 270, and medium sunshade, I get diffraction rings, with an aperture of 1·42 inch. With aperture 3·45 inches *Mercury* is perfectly clean and round, but, whether from contrast or not, the limb is decidedly brighter, or rather less dark than the centre."

$8^h\ 5^m$ sid. time. "I have just tried powers 92 and 174, and find the shading much less apparent than with a higher power. With a power of 382 it is marked. I get no trace of a bright spot."

"No satellite."

With an aperture of 10 inches and powers 244, 381 and 563, Mr. Ranyard looked carefully for rings round the planet or spots upon its disk, without seeing either. When the image of the planet was thrown on the slit of the finder spectroscope no other change was perceptible except the appearance of a black streak traversing the spectrum from end to end, caused by the interruption of the light.

Mr. H. J. Carpenter says: "I could not see *Mercury* outlined against the sky, nor detect a halo round it, nor a satellite, although I looked carefully with the 15-inch (reduced to 10 inches), as well as with the reflector."

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Dr. Copeland found the planet plainly visible with so small an aperture as 0·4 inch, but much shaded off at the limb. This seems to be the chief effect of applying small diaphragms. With an aperture of 0·95 inch some traces of a bright spot in the centre of the planet's disk were perceived. With the same aperture and telescope Mr. Ranyard thought he saw a greyish ring round the planet. It was the only diaphragm that produced any trace of a bright central spot.

Mr. J. G. Lohse, with the 4-inch Cooke, power 60, measured the distance of the centre of the planet from the Sun's limb, as follows:—

Dunecht Mean Time. h m s	Dist. ′ ″	Position Angle. °
3 7 52·2	0 20·73	45
12 50·2	0 38·84	45
16 17·2	0 56·61	45
20 38·3	1 14·45	43·7
30 18·3	1 46·44	43·7
32 20·3	1 56·83	43·7
36 31·3	2 9·35	41·73
40 26·3	2 24·56	41·73
44 8·3	2 37·15	41·73
49 30·3	3 2·97	41·73

And the diameter of *Mercury*—

1st. With the full aperture—

$$10''.87 \pm 0''.29, \text{ 5 measures.}$$

2nd. Aperture reduced to 1·7 inch.

$$11''.98 \pm 0''.16, \text{ 12 measures.}$$

Dr. Copeland, with a power of 126 on the 6·06-inch Simms object-glass, found the diameter of *Mercury* to be as follows:—

Diam. of ☿. ″	Aperture. in.	Pos. Angle. °	No. of meas.
10·84	6·06	45	2
10·73	3·60	124	4
10·53	3·60	34	8
9·40	0·95	...	1

The twelve measures with the aperture reduced to 3·6 inches give a mean of $10''.597 \pm 0''.099$, corresponding to a diameter of $5''.916$ at the mean distance of the Sun.

Mr. H. J. Carpenter, with the 12·9-inch reflector and a power of 171, obtained measures of the diameter of *Mercury* with a mean result of $12''.43$.

All the observations have been corrected for refraction where

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necessary, and the periodic errors of the screws have been eliminated.

5. *General Remarks.*

As a check on the values of the micrometer screws used to measure the size of *Mercury*, the chief pair of Σ 1090 has been measured:

1. By Mr. J. G. Lohse with the 4-inch:

Distance $60''.69 \pm 0''.39$; 12 measures.

2. By Dr. Copeland with the 6·09-inch:

Distance $60''.80 \pm 0''.11$; 11 measures.

Position $98°.08 \pm 0°.11$; 16 measures.

As a further check the same pair of stars were measured by Mr. J. G. Lohse with the Grubb 15-inch, full aperture.

Distance $60''.57 \pm 0''.12$; 9 measures.

Position $97°.56 \pm 0°.08$; 9 measures.

A combination of these determinations gives for Σ 1090

1878·37 Dist. 60″·685, Pos. 97°·73, A and B.

One measure of distance and two of position with the 6-inch gave for the third star

1878·37 Dist. 20″·82, Pos. 319°·75, B and C.

With the same telescope A was 7·9 mag. yellowish, B 8·5 white, and C 10·5 mag.

The hour angle was too great and the companion stars unfortunately too faint to be measured with the unsilvered reflector.

The focal lengths of the telescopes used are approximately as follows:—

		in.
15·06	Grubb refractor	183·2
12·9	Reflector	122·5
6·06	Simms refractor	74·5
6·04	Cooke „	90·3
4·00	„ „	59·5
3·74	Grubb finder	43·8

The best four of about twenty photographs have been measured, but the final results have not yet been deduced. It is worthy of remark that the image of *Mercury* appears much

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smaller in the photographs than it did on the ground glass of the camera.

The latitude of Dunecht by transit-circle observations is 57° 9′ 36″; the longitude, taken from the Ordnance Survey map, is 9^m 40^s west of Greenwich. The time was determined with the transit circle on several nights both before and after the day of the transit. Unless otherwise stated, the times are given in Dunecht sidereal time.

Dunecht, June 12, 1878.

Observations of the Transit of Mercury of 1878, *May* 6.

By A. C. Ranyard, Esq.

The transit was observed by me at Dunecht. Lord Lindsay kindly gave me the use of his 15-inch refractor, which was stopped down to ten inches during the observation, in order to avoid all chance of damaging the eye-pieces or dark shades by the Sun's heat. On one of the finders was a spectroscope, with a single compound prism, by Grubb, with a dispersion of 5° 56′ from *B* to *h*. On the other finder was a low-power eye-piece, with cross wires, which were brought into accurate collimation with the centre of the slit of the spectroscope. On the large instrument was a Merz polarising eye-piece, giving a power of 244.

As the planet came up to the Sun's limb, from about ten minutes before the time of predicted contact, I observed the spectrum of the sky in the direction in which the planet was approaching. The viewing telescope of the spectroscope was turned so that the red end of the spectrum below C was in the field of view, and the slit of the spectroscope, which subtended an arc of 5′ 45″ upon the Sun, was nearly at right angles to the path of the approaching planet. The slit was swept outward in right ascension over the path of the planet, and any variation in the brightness of the field corresponding to the place of the planet was carefully looked for. My reason for attempting this observation was that M. Janssen, in observing the transit of *Venus*, thought that by making use of a screen of blue glass he had been able to detect the body of the planet projected upon the background of the corona some time before it was visible without the screen. As far as I am aware, there are no observations which indicate that the spectrum of the lower parts of the corona is richer in blue rays than the spectrum of the light derived from the veil of illuminated particles floating in our own atmosphere, which under ordinary circumstances hides the corona from our view. Indeed the evidence is rather in the contrary direction, for Mr. Stone, observing during the total eclipse of April 1874, was of opinion that the continuous spectrum of the lower parts of the corona was rich in red light. I therefore

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thought it better to examine the red end of the spectrum, but I did not succeed in detecting any indication of the position of the planet before it came up to the Sun's limb. It should, however, be remembered that the sky was until first contact covered with thin clouds, which were at times sufficiently thick greatly to diminish the Sun's light.

I continued my observations at the spectroscope until some time after the signal for first contact had been given, and having satisfied myself that I could not see any longitudinal streak or variation in the brightness of the spectrum corresponding to the outer limb of the planet in the field of the spectroscope, I turned to the larger instrument with the Merz polarising eye-piece.

The planet had now entered quite one-third of its diameter upon the Sun's disk, and the sky was rapidly clearing, but I was unable to see any trace of the outer limb or any portion of the planet which was not projected upon the Sun's disk.

I then passed back again to the spectroscope and watched until the planet was about two-thirds upon the Sun's disk, but saw nothing to indicate the position of the outer limb in the field of the spectroscope.

On turning again to the large instrument with the Merz eye-piece, only a small arc of the planet remained off the Sun's disk. The sky was now clear and the definition was very good, but I could not see any trace of the part of the planet off the Sun, and I certainly could not see any line of light around the outer limb of the planet as described by some of the transit of *Venus* observers. I continued to watch with the Merz eye-piece turning the field alternately brighter and darker until the planet was entirely upon the Sun's disk, and I gave the signal for geometrical contact at the moment when the disk of *Mercury* and the disk of the Sun appeared to have a common tangent. There was no sticking or any appearance that could be described as a black drop, and in another second—certainly in less than two seconds—after the signal I saw a bridge of light separating the planet from the Sun's limbs. The time of my signal as taken down by an assistant, who, however, neglected to observe the minute, corresponds to $6^h\,[2^m]\,15^s{\cdot}5$ Dunecht sidereal time. When *Mercury* was upon the Sun's disk I carefully looked for rings round the planet and for the spot of light which has been observed in former transits upon the disk, but could not detect any trace of either the rings or the spot of light; but in observing afterwards with a cardboard diaphragm of less than an inch in diameter upon the instrument used by Dr. Copeland I thought that I at times saw a greyish ring round the planet, but did not feel quite certain as to its existence.

When the planet was well advanced upon the Sun's disk, the slit of the spectroscope was placed so as to bisect the disk of the planet, and the black band, which was then visible running longitudinally across the field of the spectroscope, was carefully examined; but neither Dr. Copeland, who assisted me in this

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observation, nor I, could detect any alteration in the solar spectrum at the edges of the band. The slit of the spectroscope was then shifted by pressure upon the side of the instrument until the slit cut off a small arc or was nearly tangential to the disk of the planet, but no changes in the solar spectrum at the edges of the band could be perceived.

The difficulties of the observation (owing to the small magnifying power and to the atmospheric tremor) were, however, so great that no reliance must be placed on this observation as affording any but the roughest evidence that the planet has not a very large absorbing atmosphere.

In the evening after the transit the collimation of the spectroscope and finder was tested upon a star and found not to have shifted, so that there can be little doubt that the slit was swept over the place of the planet as it came up to external contact.

Observations of the Transit of Mercury, 1878, *May* 6.

By Maxwell Hall, Esq.

(*Extract from a Letter to the Astronomer Royal.*)

The transit of *Mercury* was observed here yesterday under circumstances well worth your consideration. The Ingress was obscured by a cloud, but the Egress showed that no reliance could be attached to the estimated times of the various phases of contact, δ, ϵ, ζ, geometrical contact and bisection; they were all so linked together that the time of any phase was liable to an error of about 10 seconds.

A party of ladies and gentlemen assembled here to see the transit, and the results are most curious and entertaining. With a low power of 55 most of us saw a ring of light, brighter than the surface of the Sun, surrounding the black disk of *Mercury*; with a higher power of 120 some of us saw both the ring and a bright central spot, some saw only the ring, some only the spot, and some neither. With a power of 200 I myself distinctly saw the spot in the form of a *small* and *narrow ring* of light, concentric with the black disk of *Mercury*, and about 5″ in diameter; but with this power I found that the *exterior ring had vanished.*

The sketch made by Dr. Huggins during the transit of *Mercury* in 1868 (*Monthly Notices*, vol. xxix., p. 27) was carefully compared with what we saw with the intermediate power, and, excepting the eccentric position of the bright spot in that sketch, it was generally approved of as representing our combined descriptions.

There cannot therefore be the least doubt that these appearances are merely optical; and they were only seen by those who had the more delicate eyesight.

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A first-surface solar prism and a dark glass shade were employed; the diameter of the object-glass is 4 inches.

The Observatory, Jamaica,
1878, *May* 7.

Observations of the Transit of Mercury, May 6, 1878, *at Princeton, N. Jersey. Lat.* 40° 20′ 58″·0 ± 0″·2, *Long.* 9ᵐ 36ˢ ± 1ˢ *E. of Washington.* By Professor C. A. Young.

The instrument used was the new Clark E quatoreal, of 9½ inches aperture, armed with a Merz polarising solar eye-piece. The whole aperture was employed. The magnifying power at the first observation was 155; all the rest were made with a power of 223.

Flying clouds were troublesome, and the seeing was very bad, from the unsteadiness of the air, but the phenomena took place with a rapidity and decisiveness quite in contrast with what I had seen at the transit of *Venus*, so that I think the probable error of the times given cannot much, if at all, exceed one second.

The timepiece used was a sidereal clock beating once in two seconds. Its errors were carefully determined by star observations on the nights preceding and following the transit, and it was compared with the solar clock by means of the chronograph.

The solar clock was also compared with Washington time by means of the telegraphic noon signals which are sent daily to New York. By the kindness of Mr. Merrihew, the Superintendent of the Eastern Division of the Western Union Telegraph Company, these signals were transmitted over the wire which passes into the Princeton office for several days preceding the transit and on the day of the transit. The time was carried from the Telegraph Office to the Observatory by means of a pocket chronometer. The telegraphic comparisons were not, however, quite satisfactory, owing to interruptions from unskilled operators on the line, who broke it continually; but the approximate longitude stated above, and derived from these comparisons, agrees well with that deduced from the coast survey maps.

The first contact (external) at Ingress was noted at

h	m	s		h	m	s		
1	11	35·7	Sid. =	22	14	13·9	Pr. M.T.	Magnifying power 155.

The second contact (internal) at

h	m	s		h	m	s		
1	14	27·7	Sid. =	22	17	05·4	Pr. M.T.	Magnifying power 223.

At 1ʰ 12ᵐ 41ˢ·7 Sid. or 22ʰ 15ᵐ 29ˢ·7 M.T. was noted the

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Ingress of the estimated centre of the planet, but the observation was not very satisfactory.

At $1^h\ 14^m\ 34^s.7$ Sid. $= 22^h\ 17^m\ 12^s.4$ M.T. the Ingress was certainly past. During the seven seconds between this moment and that noted as the moment of internal contact there was some quivering and flickering of the luminous band between the limb of the planet and that of the Sun. The contact was noted at the instant when the black connection between the planet and Sun's limb *first* broke, which it did pretty suddenly.

At Egress the internal contact was noted at

h	m	s		h	m	s	
8	42	06·3	Sid. or	5	43	30·7	Princeton Mean Time.

The external contact (through flying clouds) at

h	m	s		h	m	s	
8	44	54·3	Sid. or	5	46	18·3	Princeton Mean Time.

Both observations were satisfactory, though the seeing was very bad. There was an interval of about 4ˢ at the internal contact, during which the band of light between the edge of the planet and the Sun was flickering, the time noted being that when several dark filaments first shot across. Two seconds earlier the band began to waver and change its appearance, and two seconds later it was quite certain that the contact was past. The external contact was equally definite.

The seeing was exceedingly bad, the limbs both of the Sun and planet being deeply serrated by the action of a strong cold N.W. wind, so that I was and still am surprised at the decisiveness and promptness with which the phases presented themselves.*

During the transit I examined the planet carefully with various powers from 100 to 500, and although the granulations of the Sun's disk often showed beautifully at moments of good seeing, I could make out nothing like a ring of light around the planet nor anything like a bright spot upon its disk. Once or twice, when the definition was specially good, I thought I could see that close around the planet there was a tendency to an apparent radiating structure of the solar "rice grains," as if the atmosphere of the planet had some elongating action upon their images. This is, however, just the opposite of the effect refraction would produce, and on the whole I fancy the apparent effect was wholly subjective.

The colour of the disk was a dark violet, exactly the same as that of a Sunspot seen with the same polarising eye-piece, or with a Herschel solar eye-piece and shade of "London smoke."

During the transit about 80 measures of the planet's diameter were made by bringing the disk between different sets of slightly

* Other observers here, with smaller telescopes of apertures between 3 and 4 inches, saw the "black drop" phenomenon most conspicuously. Dr. Alexander reports that the "black drop" at Ingress was so troublesome as to render his observation of contact entirely unsatisfactory.

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converging lines, or rather bands, photographed on glass. The bands were made wide enough to destroy the effect of irradiation upon the planet's disk. There were six different sets of bands, and the observations were made by three different persons—Mr. W. Libbey, jun., Professor Rockwood, and myself. The magnifying powers were varied from 155 to 500, most of the observations being with the highest power.

A preliminary reduction, the results of which may be slightly, but only slightly, changed by a more complete discussion, gives the diameter of the planet, from set 1, 11″·89; set 2, 11″·59; set 3, 11″·85; set 4, 11″·79; set 5, 11″·76; and set 6, 11″·56. The general mean is 11″·74. The tabular diameter of the American Almanac is 11″·99.

Princeton, May 13, 1878.

Observations of the Transit of Mercury.

By S. P. Langley, Director of the Allegheny Observatory.

(*Communicated by E. Dunkin, Esq., F.R.S.*)

The Transit of *Mercury* of May 6 was observed here under the favourable circumstance of an exceptionally transparent blue sky in the early part of the day, which enabled some observations to be made which seem worth record.

The principal instruments were an achromatic of 13-inch aperture, with a polarising solar eye-piece, which gives images free from colour without the use of shade glasses; for photometry, a Jamin photometer, used with a camera attached to the telescope; for heat, the special apparatus used heretofore on the thermal measurements of Sunspot nuclei. In the field of the polarising eye-piece was a glass reticule, ruled in squares, whose sides were 15″ each. Powers of from 120 to 800 were employed through the day at the eye-piece, and the image of the planet, greatly enlarged, was also projected on a white surface in the dark camera. The following are the chief results:—

(1) The entire disk of the planet was seen outside the Sun at about 30ˢ before first external contact, on a just perceptibly lighter background.

(2) The true diameter, which I suppose to have been seen under these circumstances, was estimated, by aid of the reticule, to be ⅛ larger than that exhibited directly after, when the planet had entered on the Sun. (1st external contact, $21^h\ 52^m\ 50^s{\cdot}43$ Allegheny M.T., by chronograph.)

(3) There was momentarily good definition at first internal contact ($21^h\ 55^m\ 47^s{\cdot}25$ by chronograph, Allegheny M.T.), but no black drop or ligament was seen by me. (An assistant, using a 4-inch achromatic, with dark glass, saw a ligament, and observed contacts from 5 to 15 seconds later.)

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(4) No bright spot or annulus was seen during the day.

(5) The planet was not seen black, as it has been often called. The light was not materially less than that of some Sunspot nuclei, but bluish grey, and measured 8 per cent. at the least of mean sunlight. (This will possibly be subject to a subsequent correction for imperfect achromatism.)

(6) There was unquestionable evidence of heat from the same direction.

(7) The planet was darker at the centre than at the edge, but in the photometric observations this was proved beyond question to be chiefly due to minute and rapid atmospheric tremor.

The following inferences seem obvious. From (1) we must conclude that the coronal background made its presence sensible without an eclipse to at least 15″ from the limb. From (2) that a correction of something like 20 per cent. is to be added to micrometric eye-measures of *Mercury* on the Sun, for irradiation. (4) The observation of these in previous transits rests on testimony not to be set aside. It may be asked, then, whether the condition of visibility is not an unusually good definition, like that enjoyed by Mr. Huggins in 1868. The definition, as regards steadiness, was here only ordinarily fair most of the day, though in the forenoon the transparency of the sky was, as has been remarked, unusual. (5) and (6) are to be interpreted, of course, not as evidence of light and heat actually radiated from *Mercury*, but as data for estimating with something like precision a lower limit for the effects of our own atmosphere in inflecting the *solar* radiation, and as giving useful quantitative results for the effects of "atmospheric glare." To make a single application: admitting that this atmospheric glare was like a bright veil, spreading eight per cent. of sunlight over both *Mercury* and the corona, and admitting also that the eye demanded at least one per cent. of excess of brightness in the background to make *Mercury* visible—we must evidently conclude that the actual brightness of the Inner Corona exceeds (at any rate) one one-thousandth of that of direct sunlight; or, as we may say in other words, the Inner Corona is several hundred times the average brilliancy of the full Moon.

Allegheny Observatory, Allegheny, Pa.,
May 13, 1878.

The Transit of Mercury, 1878, *May* 6, *observed at the Astro-physical Observatory, Ó-Gyalla, Hungary.* By Dr. Nicolas de Konkoly, Member of the R. Academy, Budapest.

(*Communicated by Mr. H. M. Christie.*)

There were five observers of the Transit: to prevent them from being under the influence of each other, they were dispersed in the Park near the Observatory.

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I observed with my 6½-inch Refractor, provided with clockwork and a polarising solar eye-piece, by Merz of Munich. My assistant, Dr. Schrader, observed the contacts with the 10½-inch Reflector Equatoreal by Browning, with a Browning double reflecting solar eye-piece; Capt. Reviczky observed with a large comet-finder, azimuthally mounted, with an aperture of 4½ inches; Mr. Alexander Cvet, from Russia, observed with a 3-inch achromatic, by Bardoux of Paris; Lieut. Horwáth with a 3½-inch comet-finder; and Mr. T. Kaiser, Engineer, with an achromatic telescope of nearly 4 inches.

After the contact observations I measured right ascension differences with Dr. Schrader. I made my measures with the 6½-inch Merz, with a Browning filar-micrometer; and Dr. Schrader measured with the Heliograph, which has an object-glass of 3 inches by Reinfelder and Hertel of Munich. This instrument is mounted and constructed only for observing the Sun, and is provided also with exquisite clockwork.

The contacts were observed as follows:—

First Contact.

Name of Observer.	Greenwich M.T. h	m	s	Ò-Gyalla M.T. h	m	s
Lieut. Horváth	3	12	10·4	4	24	56
Alexander Cvet	3	12	31·4 [?]	4	25	17 [?]
Joseph Kaiser	3	12	7·4	4	24	52

Second Contact (*Inner*).

Name of Observer.	Greenwich M.T. h	m	s	Ò-Gyalla M.T. h	m	s
Lieut. Horváth	3	14	31·4 [?]	4	27	17 [?]
Alexander Cvet	3	13	58·4 [?]	4	26	43 [?]
Joseph Kaiser	3	13	44·4	4	26	30
Dr. Schrader	3	13	45·4	4	26	31
Dr. Konkoly	3	13	45·4	4	26	31

Capt. Reviczky saw nothing of the phenomena. He had badly arranged the Barlow lens and eye-piece in the telescope supplied to him.

None of the observers saw the planet before it touched the Sun's limb, and no one observed either the "black drop" or an aureola round the planet, or any white spot upon its surface.

I measured six transits of east–west limb from Sun and *Mercury*, and my assistant, Dr. Schrader, measured ten. All transits were noted on the chronograph as follows:—

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Observer: Konkoly.

No.	Ò-Gyalla M.T.			Greenwich M.T.			Δ ☿ − ☉		No. of Webs as observed.
	h	m	s	h	m	s	m	s	
I	4	45	0·1	3	32	14·5	+0	42·64	7
II	4	56	12·7	3	43	27·1	+0	39·67	5
III	5	1	12·9	3	48	27·3	+0	38·60	7
IV	5	26	42·9	4	13	57·3	+0	32·36	7
V	5	44	0·1	4	31	14·5	+0	27·97	6
VI	5	47	53·2	4	35	7·6	+0	27·33	7

Observer: Schrader.

No.	Ò-Gyalla M.T.			Greenwich M.T.			Δ ☿ − ☉		No. of Webs as observed.
	h	m	s	h	m	s	m	s	
I	4	34	37·9	3	21	52·3	+0	45·19	3
II	4	37	18·9	3	24	33·3	+0	44·49	5
III	4	40	7·9	3	27	22·3	+0	44·07	5
IV	4	49	11·8	3	36	26·2	+0	41·50	5
V	4	52	1·7	3	39	16·1	+0	40·94	5
VI	5	12	39·2	3	59	53·6	+0	35·55	5
VII	5	15	28·1	4	2	42·5	+0	35·09	4
VIII	5	30	54·7	4	18	9·1	+0	31·10	7
IX	5	33	55·3	4	21	9·7	+0	30·23	7
X	5	59	13·1	4	46	27·5	+0	24·07	6

The differences are yet free [*sic*] from atmospheric refraction. I have also given Greenwich mean time to facilitate the comparison of my observations with English observations. The Δ ☿ − ☉ is mean solar time.

The Heliograph is a new instrument, installed on the 1st of November of last year in the smaller drum of the Observatory. It is mounted equatoreally on a stone pillar, with driving clockwork apparatus. There is no continuous tube, but a framework of steel and brass rods for supporting the object-glass and the eye-glass: the Sun's image is more steady than in an instrument with a wooden or brass tube. The object-glass is one of the best quality, made by Reinfelder and Hertel of Munich.

The Ò-Gyalla Observatory is $1^h\ 12^m\ 45^s{\cdot}59$ E. of Greenwich; the latitude is $47°\ 52'\ 43''{\cdot}0$ N.

Ò-Gyalla Observatory,
1878, June 8.

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On the Mean Solar Parallax as derived from the Observations of the Transit of Venus, 1874.

By Captain G. L. Tupman, R.M.A.

(*Communicated by the Astronomer Royal.*)

In order that astronomers may judge of the manner in which I have treated the question in this paper, and form an opinion of the value of the result obtained, it is necessary that they should be in possession of the original observations. Those made by the British Government Expeditions were published in detail in the Report presented to Parliament by the Astronomer Royal, and are supposed to be sufficiently accessible, especially as the important parts of them are quoted in Mr. Stone's Paper in the *Monthly Notices* for March last (vol. xxxviii., p. 279). The following are similar verbatim extracts from all the other reports received up to the present time at the Royal Observatory.

Mr. Stone, Her Majesty's Astronomer at the Cape of Good Hope, observed with the 7-inch Equatoreal, power 200, shade not stated. The sky was perfectly clear of cloud; there was no wind, and the definition was good.

Egress.

The times near contact were taken down from a chronometer by Mr. Freeman, and, to avoid all possible effects of bias, no attempt was made by the observer to count the seconds between the different phenomena observed. As *Venus* approached the Sun's limb, signals were given at different intervals indicating that no disturbance of the Sun's limb near the point of contact had taken place.

	h	m	s	
At	19	6	25·0	Cape mean time, some disturbance of the limb was first suspected. An examination immediately after this showed that the suspicion was unfounded. There was no appearance of any ligament.*
At	19	7	12·0	the connexion between *Venus* and the Sun's limb was first established. It was now seen continuously, and of a fair breadth.†
At	19	7	29·6	apparent contact not perfect; but the ligament of considerable breadth.
At	19	8	5·0	apparent contact.

* This is the language in which Mr. Stone's observation was originally communicated to the Astronomer Royal. In his paper in the *Monthly Notices* for April last Mr. Stone has given $19^h\ 6^m\ 25^s$ as a *good* observation.

† It must be kept in mind that *breadth* applied to a ligament, shadow, or connection between the limbs is measured in a direction parallel to the limbs; but when applied to the ring of light round the planet, or to the faint band joining the cusps of the Sun, it is measured perpendicular to the limbs—G.L.T.

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"With respect to the first contact I should not be surprised to find that observers with less powerful optical means, or who have employed denser absorbing media to reduce the brightness of the image than employed by me, may not have observed the ligament at so considerable a distance between the apparent limbs of *Venus* and the Sun as I have done; but there is no doubt whatever about the ligament having been seen by me continuously from the time noted, $19^h\ 7^m\ 12^s{\cdot}0$.

The time given for apparent contact is when the curvature of the limbs, those of *Venus* produced, appeared to touch. It was exceedingly difficult to fix the times with great certainty on account of the very slow changes in the form depending on the oblique motion of *Venus* across the disk."

Mr. Finlay, the First Assistant at the Cape, observed with the 4-inch Refractor, power 180.

		h	m	s	
Real	contact at *egress* at	19	7	11·0	Cape mean time.
Apparent	„ „	19	8	18·5	„

"The time noted as that of 'real contact' is when I first noticed a slight connection between *Venus* and the Sun, while *Venus* still seemed to be within the Sun's disk. At this time the ligament was about as broad as it was long.

This ligament continued to increase in breadth till the time noted as that of 'apparent contact,' when the limbs were considered to coincide. I found some difficulty in judging this time accurately, and was inclined to give the signal for it more than once before the time noted: the contact was decidedly established when I did give the signal. From recollection I should say that at this time the ligament was rather less than a semi-diameter of *Venus* in breadth. Probably the observation is several seconds late."

[The Cape observers do not mention, nor do their diagrams show the illumination in the planet's atmosphere joining the cusps of the Sun.]

Mr. Maclear, observing with the Transit Theodolite by Troughton & Simms, aperture 1·65 inch, diagonal eye-piece, power 23, recorded "first appearance of actual permanent connection between the dark limb of *Venus* and the Sun" at $19^h\ 7^m\ 48^s$ Cape mean time.

Mr. H. L. Spindler observed at Fort Frederick, Port Elizabeth, latitude 33° 57′ 59″ S., longitude $1^h\ 42^m\ 28^s{\cdot}8$ E. (by the Cape Trig. Survey), with a $3\frac{1}{2}$-inch achromatic by Troughton & Simms, "showing the Sun direct image." Power and colour of dark glass not stated. Time was communicated by telegraph from the Cape Observatory.

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Egress.

h m s
At 19 7 7·5 *Cape* mean time the contact seemed so nearly perfect that only a thin dark line along the edge of the Sun proved the contact hardly consummated.

[In reply to a query from Mr. Stone, dated 1875, March 5, three months after the transit, Mr. Spindler explains that he unthinkingly used the word "dark" line instead of "bright" line, and he further states that "the last time a clear *bright* line of division was perceptible would be about 5 seconds before the first time recorded," or at $19^h\ 7^m\ 2^s{\cdot}5$ C.M.T.]

h m s
At 19 7 17·5 the appearance of the limbs was as though blended in one sharp defining line, hence set down as the moment of perfect contact.

At 19 7 33·0 the contact may be described simply as fully perceptible or certain, but with no observable loss of circular form in any appreciable degree. I believe, on a reconsideration of all the circumstances, a few seconds added to the second time ($19^h\ 7^m\ 17^s{\cdot}5$) will be the true moment of apparent internal contact.

Mr. D. Gill, of Lord Lindsay's expedition, observed at Belmont, Mauritius, latitude 20° 2′ 57″ S., longitude $3^h\ 50^m\ 41^s{\cdot}0$ E., (Gill) with a 4-inch achromatic, by Cooke, and Merz polarising eye-piece, power 150.

Egress.

"The internal contact was observed at $21^h\ 48^m\ 12^s{\cdot}5$ local mean time. A few seconds previous to what was noted as contact a light grey shade appeared between the limbs of the Sun and planet. This appeared to become black almost at the same time as geometric contact. I only observed this time; but I think it may be four or five seconds in error, as there was no precise phenomenon to observe."

Dr. R. Copeland, at the same place, observed with a 6-inch achromatic, solar diagonal, power about 100. Sun's limb boiling.

"Last internal contact—formation of ligament seemed identical with geometrical contact, and can scarcely be uncertain to more than two seconds—at $21^h\ 48^m\ 11^s{\cdot}3$ Belmont mean time."

Mahmoud Bey, Astronomer to his Highness the Khedive, at the Observatory at Abbasseyeh, lat. 30° 4′ 38″·1, long. $2^h\ 5^m 8^s{\cdot}9$ E. (Browne), observed with an Equatoreal of about 6 inches aperture.

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Egress.

	h	m	s	
Distinct formation of ligament	20	11	13·2	Local mean time.
Internal geometrical contact	20	12	18·7	„ „

Colonel TENNANT observed at Roorkee, in lat. 29° 51′ 33″·8 N., long. $5^h\ 11^m\ 33^s{\cdot}4$,* with a 6-inch Equatoreal by T. Cooke & Sons, fitted with a solar diagonal.

The whole of the time the tremor was very great.

"The *Ingress* was observed with the double-image micrometer eye-piece, power 128, neutral tint glass and solar diagonal. The contact of limbs was observed at $12^h\ 42^m\ 26^s{\cdot}1$ local sidereal time. Possibly there was some confusion from the two images, though *one* was clear, and there was a good deal of atmospheric disturbance.

"I saw no peculiarity: the contact took place without any black drop or distortion. At the moment it occurred I was endeavouring to get hold of the tappet or key, and in doing so lost count; but I looked up at once at the dial (and allowed for lost time) when I was sure that the critical phenomenon had really passed without any of the peculiarities I had been led to expect. This estimated interval was three seconds, and I think that all causes of error would be included in ascribing a probable error of two seconds to the observation of time.

"The *Egress* was observed with a Huyghenian eye-piece, power 205, with yellow-green (chlorine) glass on the diagonal prism. The final line of light was very fine, and I can only describe its disappearance by saying that it melted away. The vanishing occupied a sensible portion of time, but very small. It was too short for me to be able to say that there was any veil or lessening of the brilliancy of the line of light. There was no black drop or distortion; I saw no projection of *Venus* on the background. The contact was observed at $16^h\ 28^m\ 36^s{\cdot}1$ local sidereal time."

Captain STRAHAN† observed at Lahore, latitude 31° 23′ 23″·5 N., longitude $4^h\ 57^m\ 20^s{\cdot}1$ E., with a 6-inch Equatoreal by Troughton & Simms, negative power 125, on solar diagonal.

Egress.

"As the planet moved towards the Sun's limb, she appeared to push away his edge before her, the cause of which became evident in a few seconds; the planet's edge was, in fact, encircled

* Adopting General Addison's correction to the old longitude of Madras, which is the point of reference of all the longitudes of the Great Trigonometrical Survey.—G.L.T.

† Col. TENNANT's Report, p. 35.

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by a ring of light nearly as bright as the Sun, which prevented any contact, properly so called, from taking place at all. The moment I have assumed for internal contact, and recorded as 'slight darkness between the limbs,' was when the Sun's light, unbroken by the ring of light, would just have grazed the limb of *Venus*. This was at $16^h\ 14^m\ 36^s{\cdot}9$ local sidereal time; some uncertainty about the minutes; seconds correct; no black drop; the darkness enveloped more of the limbs without increasing in depth (of shade); no dark ligament occurred at all. I have considerable confidence in the accuracy of this observation, as the limbs were beautifully steady and sharp, and no distortion apparent."

Captain A. C. Bigg-Wither observed at Mooltan, latitude 30° 11′ 5″ N., longitude $4^h\ 46^m\ 1^s{\cdot}8$ E.,* with a 4-inch achromatic by Cooke, solar diagonal, power 55 for *Ingress*.

"The Sun rose with the planet about half-way on to its disk, and even at internal contact it was too near the horizon to be well seen; with power 55, however, I satisfied myself that the 'black drop' was formed, the apparent internal contact being at about $12^h\ 14^m\ 42^s$ Mooltan sidereal time, the black band joining the planet's disk with the darkness of space being then about 20″ broad; the real internal contact was at $12^h\ 17^m\ 8^s$ M.S.T. about, the black band with a breadth of about 7″ being then apparently broken."

Egress.—"As *Venus* approached the Sun's limb, she appeared to push before her a ring of light concentric with her disk; there was absolutely no internal contact whatever, no formation of a black drop, and no distortion of the apparent shape of the planet's disk."

Mr. Hennessey† observed at Mussoori, in the Himalayas, latitude 30° 27′ 36″·3; longitude $5^h\ 12^m\ 12^s{\cdot}2$ E, and height above the sea 6,765 feet, with a 5-inch Equatoreal, power 125.

At *Ingress* the Sun was $7\frac{1}{2}°$ above the horizon; internal contact was noted at $14^h\ 17^m\ 9^s$ *Greenwich* mean time.

Egress was observed with a red glass. Neither pear-drop nor ligament, nor any other connection or shadow, appeared between the limbs, until at last the two fairly touched at $18^h\ 5^m\ 32^s{\cdot}6$ G.M.T.

General Addison observed at Kurrachee, lat. 24° 50′ 58″·4, long. $4^h\ 27^m\ 53^s{\cdot}4$ E. (*Monthly Notices*, vol. xxxviii., p. 83), with a 5-inch Equatoreal by Cooke, power 117.

	h	m	s	
Internal contact at *Egress*	15	44	10·9	Local sidereal time.

* Captain Bigg-Wither gives no authority for the longitude. Supposing it to be taken from the Indian Survey, I have increased it 2″·3 (Addison's correction). How the local time was obtained is not stated.—G.L.T.

† *Proceedings of the Royal Society*, No. 159.

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From the MADRAS Observatory, lat. 13° 4′ 8″·1 S., long. $5^h\ 20^m\ 59^s{\cdot}6$ E. (Addison), Mr. Pogson reports of *Egress*:—

"Just about 3rd contact the clouds grew a little thinner, and the contact was noted by

	h	m	s	
Miss E. J. Pogson at	23	24	32	Madras mean time.
C. R. Chary at	23	24	46	,, ,,
T. M. Pillay.	23	25	12	,, ,,

probably some mistake about the last.

"The two first observers both agreed that no black drop was visible, but through such clouds I only wonder they saw the contact at all."

Mr. A. V. N. Row observed at Vizagapatam, lat. 17° 42′ 9″ N., long. $5^h\ 33^m\ 30^s$ E. (authority not given, probably depends upon the Trig. Survey, and I have increased it to $5^h\ 33^m\ 32^s{\cdot}3$), with a 6-inch Equatoreal by Cooke, solar diagonal *and double parallel wire micrometer with its eye-piece No.* 2. *One of the wires was kept a tangent to the Sun's limb at the point of contact*, and in this way the internal contact at *Egress* was noted at $16^h\ 47^m\ 15^s{\cdot}4$ local sidereal time.

At Melbourne Observatory, Mr. ELLERY observed with the 8-inch Equatoreal by Troughton & Simms, negative eye-piece of 125, with Herschelian prism and yellowish smoke-tint dark glass. Sun free from clouds.

Ingress.

h	m	s	
At 23	59	39	Melbourne mean time. Apparent coincidence of limbs of Sun and *Venus*, or tangential contact.
At 0	0	10·9	"Chinaman's Cap," an appearance generally seen with the artificial transit, but was only faintly seen for an instant in the actual transit; it is like a very obtuse triangle with its broad base towards the Sun's limb, whilst its apex touches *Venus*, the two similar sides being curved towards the base and towards each other.
At 0	0	26·0	first appearance of occasional flicker of light between the limbs. A streak of light was occasionally seen to glimmer clear between the planet and the Sun's limb; this soon gave way to the next appearance.
At 0	0	44·8	appearance of smoky junction of limbs, a broad band as if of thin smoke occupied the space between the limbs. This gradually gathered up as it were, till it became like a very thin thread, which was now and then invisible, although the limbs of planet and Sun were very well defined and steady. It appeared sharper and thinner at last.
At 0	1	9·4	sudden disappearance or rupture of the very thin dark thread joining the limbs.

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"In these observations I was surprised at the absence of any (except a mere trace) of the appearances nearly always seen in the artificial transit, and which were fully expected in the actual transit. The phases were all clear and sharp, but the motion of *Venus* being so slow and oblique, it impressed me that it was impossible to assign the time of the true internal contact to within two or three seconds of time with any certainty, even when the edges of the two bodies were almost perfectly quiescent and as sharp as two disks of metal."

Egress. Eye-piece the same as at Ingress.

	h	m	s	
At	3	26	57·6	Melbourne mean time, haze or smoky appearance first seen to join limbs of Sun and *Venus*. The smoky junction appeared very faint at first but unsteady as regards width, sometimes narrow, at other times as wide as one-third the diameter of the planet. It was never black, but only like a thin smoke.
At	3	28	14·0	first saw a distinct thread in centre of haze. The thread was much darker than the smoke, less defined and more nebulous than at ingress.
At	3	29	3·7	tremulous junction of limbs, due to the disturbed state of the atmosphere, and boiling of the edges of the Sun and *Venus*, whereby they appeared at one instant joined and then separated, a slight haze always appearing at the separation.
At	3	29	20·0	distinct junction of limbs, when they no longer appeared to separate by the tremulous motion.

Mr. WHITE, the First Assistant at Melbourne Observatory, observed as follows, with an *Altazimuth* of $2\frac{3}{4}$ inches aperture, with 4-glass diagonal positive eye-piece and red glass shade.

[Power not stated, but it would probably be only 30 or 40.]

Ingress.

	h	m	s	
At	23	58	42·	Melbourne mean time, estimated tangential contact. *Venus* appeared to be attached to the Sun by a very broad ligament nearly as black as the planet itself.
At	23	59	46·	"Chinaman's Cap." At this time the ligament narrowed very much on the side of *Venus*, the broad part being still attached to the Sun's limb.
At	0	0	44·	bright thread of light, estimated contact. A bright arc of light appeared between the broad part of the "cap" and the Sun's limb. The Sun was now clouded for 17 seconds, and when the cloud had passed, the "cap" had disappeared and *Venus* had become truly circular, and was well separated from the limb of the Sun.

Egress.

At	3	28	24·	cap thrown out. A dusky triangular figure was thrown off from *Venus*, the apex being towards the planet and the base towards the nearest part of the Sun's limb. The "cap" in this instance did not appear to be nearly so black as at ingress.

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h m s
At 3 29 15· thread of light destroyed (contact). The cap came into contact with the Sun's limb, at the same time the apex had continually increased in breadth.

At 3 30 16· tangential contact estimated. Similar to ingress.

The late Professor WILSON observed at Mornington, about 30 miles from Melbourne, in lat. 38° 13′ 54″ S., long. $9^h\ 40^m\ 8^s{\cdot}73$ E. (Ellery), with a 4½-inch Equatoreal, negative eye-piece of 145, with Herschelian prism and neutral tint wedge. Time communicated by telegraph from Melbourne Observatory.

"The Sun was clouded all the time, and I had to keep the wedge constantly working from end to end. Where the clouds were thinnest the definition was very good, and there was no boiling edge. *The times recorded are not epochs selected during a continuous sight of the ingress, but the phenomena seen at glimpses obtained at intervals.*"

Ingress.

h m s
At 23 59 43·7 Melbourne mean time, circle of *Venus* tangent to Sun's limb.

At 0 0 39·5 broad band.

At 0 1 23·4 quite clear of limb. Only an instantaneous view through a great deal of cloud. The fluffy connection seemed gone, but might perhaps have been still there if a clear view had been obtained.

Egress.

The sky was clear, there was much boiling, and the definition was not nearly so good as at ingress.

h m s
At 3 27 36·2 first appearance of a small dark object flickering backwards and forwards between the limbs. This increased, and there was no other phase to which I could attach a definite time.

At 3 29 31·4 I gave a signal, and named it "broad band," but the flickering object had gradually changed into this. *Venus* never assumed the appearance of being a tangent to the Sun's limb as at ingress.

Mr. C. MOERLIN, Second Assistant at Melbourne Observatory, observed at View Hill, Sandhurst, about 86 miles N.W. of Melbourne, in lat. 36° 48 16″·9 S., long. $9^h\ 37^m\ 12^s{\cdot}8$ E. (Ellery), with a 6½-inch Equatoreal, negative eye-piece of 120, Herschelian prism, and neutral tint wedge. Time telegraphed from Melbourne.

Ingress lost through clouds.

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Egress.

The Sun's limb and the planet appeared sharp and well defined through thin cloud. The streak of light between them, distinct and unmistakable, became smaller, and

h	m	s	
At 3	29	36·6	Melbourne mean time, all at once a sort of triangular shaped connection between the two took place, an appearance I have seen with the artificial transit, but to a more limited extent, the base of the triangle on the Sun's limb and the apex on the planet. The difference in the appearance from that which I have seen with the artificial transit consists in the much larger base. The planet every once in a while jumped off the apex of the triangle, and a thin line of the Sun's disk could be distinctly seen between the two, the distance, however, between the triangle and the planet, when jumping, growing less. I now looked out for tangential contact, watching at the same time the gradual diminution of the separation between the planet and the apex of the triangle. The clouds became thicker, but still permitted me to see the phenomenon distinctly.
At 3	30	0·1	tangential contact. The jumping or separating of the apex of the triangle and the planet ceased a few seconds before what I considered the tangential contact.

Mr. Gilbert, Fourth Assistant at Melbourne Observatory, observed at Glenrowan, about 150 miles N.E. of Melbourne, in lat. 36° 28′ 4″ S., long. $9^h\ 44^m\ 53^s{\cdot}3$ E. (Ellery), with a 4½-inch Equatoreal by T. Cooke & Sons, negative eye-piece of 120, Herschelian prism, and neutral tint shade. Time by telegraph from Melbourne.

Ingress.

h	m	s	
At 23	58	53·	Melbourne mean time, the ligament commenced to form. The Sun now disappeared behind a cloud for about 20 seconds.
At 23	59	59·	the ligament was well defined.
At 0	0	34·	apparent contact of limbs.
At 0	0	42·5	the ligament snapped suddenly.
At 0	0	50·	a clear line of light between the limbs.

"The snapping of the ligament I consider an excellent observation, the objects being so well defined."

Egress lost through cloud.

Mr. Morris observed at the same place as Mr. Gilbert, with an 8½-inch Browning-With Reflector, negative eye-piece of 150, Herschelian prism. [Shade not stated.]

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	h	m	s	
At	23	57	17·	first noticed the ligament. It became more distinct, and was not so dark as the planet itself. It gradually lessened in width.
At	0	0	45·8	it was so narrow and filmy that it was scarcely perceptible. Before the rupture was observed clouds obscured the Sun. The ligament seemed in fact to melt away gradually.

Mr. Russell, at the Sydney, N.S.W., Observatory, lat. 33° 51′ 41″·1 S., long. 10h 4m 47s·3 E. (Stone, *Monthly Notices*, xxvii. 299), observed with the 11½-inch Equatoreal, by H. Schröder, with the aperture reduced to 5 inches, direct vision eye-piece of 100 (in preference to the reflecting eye-piece). The coloured glasses were green before the eye-piece and dark blue next the eye.

[It is inferred that all the times inserted in the reports of the Sydney observers are Sydney mean times, although it is nowhere so stated.]

Ingress.—Sky clear, definition good.

	h	m	s	
At.	0	20	0·	[Sydney mean time] indications of distortion, or bad definition of the limbs in contact appeared, like a mass of black wool laid over the place, rendering it impossible to see distinctly, and making the cusps very hazy. I thought the drop was going to form, and watched very closely for it, and for apparent contact; but I found it extremely difficult to make up my mind about the latter, and saw nothing of the former.
At	0	20	51·	was noted a very unsatisfactory apparent contact. The cusps after this appeared to clear up or improve in definition, and as they approached each other the sharpness was very remarkable, but the motion was so gradual I could not determine to a fraction of a second when they actually formed the line of light which I saw complete and took for the moment of internal contact; but the instant I was sure of the line I made the record on the
	0	23	59·	chronograph (0h 23m 59s), and keeping my eye steadily
	0	24	14′	upon it, saw it had, in 15 seconds, become a distinct and unmistakable band of sunlight.

Egress.—Sky clear, definition good generally, at times bad.

	h	m	s	
At	3	53	53·6	the limb of the planet nearest to the Sun's limb seemed to be in a state of vibration, as if portions of its blackness were jumping over to the margin of the Sun. This lasted only a few seconds, the vibrations being estimated at 6 or 7 per second; after this the limbs recovered their perfect definition, and were clearly and steadily separated
	3	54	26·3	by a line of light, which, at 3h 54m 26s·3, could not have been more than half a second of arc in thickness, and then, the same marvellous definition continuing, just when it was wanted, the line gradually contracted to a scarcely
	3	54	39·7	visible thread and the limbs made contact at 3h 54m 39s·7. There was no sudden break; nothing but the perfectly gradual motion of the one disk over the other, both beautifully defined. I have no doubt that had the bad definition continued, the moment when the jumps were seen would have been taken as the formation of the drop.

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"A curious phenomenon then presented itself, similar to that remarked at ingress; the two limbs at the point of contact seemed to get confused or badly defined, whether from atmospheric causes near us or from some peculiarity about *Venus* I am unable to say, but it seemed to distort the planet in a remarkable way, and now, at Ingress, I found it very difficult to determine apparent contact; or what, under the strange effect, seemed like the phenomenon known by that name. At $3^h\ 54^m\ 45^s$ [$3^h\ 55^m\ 45^s$ in the margin, G.L.T.] it appeared to me that if *Venus* could be made round it would be just in contact with the Sun's limb.

Mr. H. A. LENEHAN, First Assistant at the Sydney Observatory, observed there with an Equatoreal, by Troughton & Simms, of $4\frac{3}{4}$-inches aperture, stopped down to 4 inches, power 80. [No mention of prism or shade.]

Ingress.

"At the time of internal contact there was an indistinct shading between the supposed edge of the planet and the Sun, which, for some 10 or 15 seconds before the time I quote below, kept me in a state of uncertainty as to the true time of actual contact. The shading did not break abruptly, but seemed to melt away in such a manner as to leave a doubt in my mind of the exact time the planet passed the edge of the Sun, but I distinctly saw a clear band of light at $0^h\ 24^m\ 48^s.3$.

"The *Egress* was observed under very favourable circumstances. There was a similar indistinctness between the time I mention as 'apparent contact' and 'complete contact,' as noted above at ingress. The first apparent contact was at $3^h\ 54^m\ 21^s.6$, a little jumping; afterwards saw a band or narrow streak of light between the limits of the planet and Sun, which clearly showed the time given ($21^s.6$) was in error. I then waited until I was *absolutely certain* contact was completed, and the time named as complete contact, $3^h\ 54^m\ 46^s.6$, I feel certain is from 7 to 10 seconds too late; thinking the light might again show between [the limbs] made me feel quite certain before I noted the second time."

Dr. H. G. A. WRIGHT observed at a station 1,950 feet south and 792 feet east of the Sydney Observatory, with an $8\frac{1}{2}$-inch Browning-With Reflector provided with one of Browning's solar eye-pieces (having two prisms arranged for single reflections), to which was added a positive eye-piece, by Ross, of 175, and a light smoke-coloured glass. The aperture was reduced to $5\frac{1}{4}$ inches. It is not stated how accurate time was obtained.

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Ingress.

"The margin of *Venus*' disk continued sharply and beautifully defined as she passed on to the Sun. A slight shimmering of the solar edge might be observed at the moment when the two outlines, as it were, of the Sun and planet touched, and also some slight shading of the planet's edge (something like a penumbra), but it was clear to me that there was no 'black drop,' nor any such elongation or distortion of the black edge of *Venus* that could be taken for it. I was so intent upon observing this that I allowed some seconds to elapse before I recorded an observation of the internal contact at $0^h\ 24^m\ 30^s$. This, I have no doubt, was half a minute after actual contact, and that it was so is proved by Mr. Russell's observation at $0^h\ 23^m\ 59^s$.

Egress.

"Determined to take the most accurate observations possible of the internal contact at egress, I carefully set my chronograph by the chronometer, and was fortunately enabled to catch the precise moment of contact, at $3^h\ 54^m\ 39^s{\cdot}6$ Sydney mean time, the edge of the planet coming at that instant sharply and distinctly in contact with the Sun's limb. It was quite as clear as at ingress that there was no 'black drop.'"

Mr. F. Allerding observed at a station 1,980 feet south and 1,450 feet east of Sydney Observatory, with a telescope of $3\frac{1}{2}$-inches aperture, *stopped down with a cardboard cap to 2 inches*. It is not stated how accurate time was obtained.

"At internal contact at *ingress*, I saw a drop which formed into a cone, and when it (the cone) had nearly disappeared, it seemed to stretch out to a fine thread, to which *Venus* seemed to be attached, reaching to the Sun's limb; then the line instantaneously disappeared at $0^h\ 24^m\ 44^s$ [Sydney mean time?], but *Venus* was then already well detached from the Sun's limb. Had I not waited for the disappearance of the fine line, I would have made the internal contact *at least* 30^s *sooner*.

"I cannot be very certain about the internal contact at *egress*, having had many interruptions, but I give the time as nearly as possible at $3^h\ 54^m\ 35^s$."

Mr. L. A. Vessey observed at Woodford, N. S. W., in lat. 33° 43′ 59″ S., long. $10^h\ 1^m\ 52^s{\cdot}7$ East (Russell) with a $4\frac{3}{4}$-inch Refractor by Schroeder, stopped to 4 inches, equatoreally mounted, diagonal eye-piece; power 96, with greenish neutral glass. Time communicated by telegraph from Sydney Observatory.

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"The times recorded for the internal contacts are very unsatisfactory; the observer was tired and nervous, and at Ingress the telescope was shaking in the wind.

Ingress.

	h	m	s	
Apparent internal contact estimated at	0	23	47·1	Sydney mean time.
Ingress complete some time between	0	24	39·4	and $0^h\ 24^m\ 47^s{\cdot}4$.

"The telescope was much shaken by the wind at $0^h\ 2^m\ 40^s$ and afterwards, and the ring of light around *Venus* cut through what would otherwise, I think, have been a drop, lasting 40 seconds if not a whole minute."

Egress.

"The Sun's light diminished till nothing but the silvery ring of light around the planet was left, and this ring was carried out on the sky by the slow motion of the planet.

	h	m	s	
About	3	54	25·5	or $3^h\ 54^m\ 26^s{\cdot}5$ Sydney mean time, the light between the limbs of the Sun and the planet became so thin that in the observer's estimation all direct sunlight was cut off, and nothing but the ring of light around the planet remained; or, in other words, the narrowing of the line of light between the sky and the planet then seemed to cease.
At	3	54	37·5	the cusps of the Sun's limb were distinctly separated, but connected by the fine ring of light around the planet.
At	3	54	48·4	(apparent internal contact) the circles of the Sun and planet were tangential, and the ring of light about its own thickness outside the limb of the Sun.
At	3	55	3·4	the apparent internal contact was decidedly passed, the ring now showing as an arch upon the Sun's limb.

"In my report I have unconsciously adopted the phraseology I had become accustomed to when using the artificial transit."

Professor LIVERSIDGE, of Sydney, N. S. W., University, observed at Goulburn, a town situated on the mountains, 2,300 feet above the sea,* with an Equatoreal of $3\frac{1}{4}$ inches aperture, power 150, with dark red glass screwed on to the eye-piece in the ordinary way. Time by telegraph from Sydney.

Ingress.

	h	m	s	
At	0	19	28·5	Sydney mean time, I was inclined to think that there was "apparent internal contact," as seen in the artificial transit.

* The exact position of Goulburn is not communicated. From a map of the colony it appears to be 58′ S. of Sydney and $6^m\ 28^s$ W. I have assumed it to be in lat. 34° 50′ S., long. $9^h\ 58^m\ 19^s$ E. If this position should be as much as five miles in error the equations would be very slightly affected, as Sydney mean time was telegraphed to the observers.—G. L. T.

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h m s
At 0 20 27·5 I again judged that there was "apparent internal contact," and considered that the former estimate was too soon.

At 0 21 29·0 the circle of *Venus* appeared to be complete and apparently just touching the Sun's limb; i.e. "internal contact" really took place.

At 0 25 1·5 the disk of *Venus* was clear of the Sun's limb, and appeared to be about one-sixteenth of the planet's diameter within it.

"During the interval between the times $0^h\,21^m\,29^s$ and $0^h\,25^m\,1^s$ a faint hazy grey filament like a streak of smoke was momentarily observed between the edge of the planet and the Sun; it was very obscure and ill defined. I unfortunately failed to note the exact time at which this cloudiness was present between the two limbs, for while trying to get it more in focus and more sharply defined, it vanished. There did not appear to be any sudden break in it, but it faded away quite imperceptibly. No traces of the black drop were seen unless the above be considered such."

Egress.

h m s
At 3 46 3·3 Sydney mean time, I fancied I could see a slight haziness between the planet and the solar limb. I do not attach any importance or value to this observation, as the haziness was exceedingly ill defined.

At 3 54 20·4 contact between the two limbs took place.

At 3 59 25·3 the planet was just beginning to pass off the Sun's limb, and it looked somewhat as if it were pushing that portion of the Sun's limb before it, for the solar limb appeared to be raised up into two processes, one on each side.

Captain Onslow, M.P., also observed at Goulburn. No mention is made of the instrument or power. Mr. Russell states that a $3\frac{3}{4}$-inch Equatoreal was sent to Goulburn, and most likely Captain Onslow and Mr. Tornaghi observed with this instrument.

Ingress.

h m s
At 0 21 29·3 Sydney mean time, *Venus* apparently just touching inner edge of Sun.

At 0 23 29·3 the internal contact appeared complete, but at this moment the objects got out of my field of vision, and when again sighted, at $0^h\,25^m\,29^s{\cdot}3$, *Venus* was well inside the Sun's rim.

Mr. Tornaghi observed the *Egress* with the instrument that Captain Onslow had used at Ingress.

h m s
At 3 53 44·8 Sydney mean time, the light between the edge of the Sun and the external edge of the planet was a little dim, though the circles were quite distinct.

At 3 54 25·8 time of contact. I took the time when the line of light between *Venus* and the Sun's limb disappeared.

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Captain Hixson, R.N., President of the Marine Board, also observed at Goulburn, with a 6-inch Equatoreal, power 130.

Egress.

h m s
At 3 54 28·0 I judged that contact took place. Previous to this there was a portion of light visible between the planet and the Sun.

*** It should be stated that none of the telescopes used at Goulburn were provided with proper means for observing the Sun. The Sun was near the zenith at Ingress, the thermometer in the shade stood at 104° Fah. All Captain Hixson's dark glasses split, causing him to lose the Ingress entirely.—G. L. T.

The Rev. W. Scott observed at Eden, N. S. W., latitude 37° 3′ 47″ S., longitude $9^h\ 59^m\ 36^s{\cdot}25$ E. (Russell), with 7½-inch Equatoreal; power and shade not stated. Time was communicated by telegraph from Sydney Observatory. *The aperture was reduced to 2 inches.*

Ingress.

"I saw the partial obscuration of the Sun's limb by the planet's atmosphere gradually diminishing until it disappeared altogether at $0^h\ 24^m\ 48^s$ Sydney mean time."

Mr. McDonnell observed at the same place with 4½-inch Equatoreal by T. Cooke & Sons; power 98, diagonal reflector and light bluish glass.

h m s
At 0 19 24·0 Sydney mean time, apparent contact was noted. As *Venus* proceeded the shadowy envelope disappeared, except between the planet and the Sun's limb, where it seemed to fill up the space between them with faint rings concentric with the planet's edge. There was no distinct rupture of this appearance, the light seeming to go in and out several times and prevented any accurate notation of the complete ingress. I
0 25 14·7 marked $0^h\ 25^m\ 14^s{\cdot}7$ as the time, but feel now convinced that it took place at least 15^s earlier. There was no abrupt breaking of the "ligament," if it can be so called, but a gradual dissolving away.

The *Egress* was lost through clouds.

Mr. John Tebbutt, at his private Observatory, Windsor, N. S. W., lat. 33° 36′ 30″ S., long. $10^h\ 3^m\ 15^s{\cdot}7$ E. (*Nautical Almanac*) observed with an Equatoreal of 4½ inches aperture (not reduced), diagonal prism, negative eye-piece of 120. The lens next the prism was smoked, a neutral tint glass, also smoked, was secured to the eye end of the eye-piece, and, in addition, another glass of very light neutral tint was occasionally employed. By this combination the Sun's limb appeared of a straw colour and the sky intensely black.

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Ingress.

"* * * A few seconds afterwards it was obvious that the apparent contact had taken place, the planet's limb being seen slightly within that of the Sun, but connected with it by a dusky ligament, whose breadth was about one-fourth the diameter of the planet. Instead, however, of the solar cusps being squared off by this ligament, they were rounded, the convexity being towards the ligament. In other words, the ligament was contracted in the middle. It was of a lighter shade across the middle between the rounded ends of the cusps than it was along the limbs of the Sun and planet. The more deeply shaded portion of this ligament resembled in colour the limb of the Moon when entering the Earth's dark shadow during eclipse. The distance between the limbs of the Sun and planet—for the solar limb at the point of contact could be faintly seen—did not at this time exceed a second of arc. During two or three beats of the half-seconds chronometer I noticed that the ligament was affected with a vibratory or tremulous motion in the direction of the line joining the centre of *Venus* with the point of contact. At the same time it resolved itself into several dusky streaks parallel to the limb of the Sun. These were at once quickly absorbed, partly into the black disk of *Venus* and partly into the dark margin of sky along the Sun's limb. The planet was then seen slightly within the solar limb, with the space between the two limbs as bright as the other portion of the Sun's disk. The disappearance of the dusky streaks, which phenomenon I regard as the real internal contact, occurred at $0^h\ 22^m\ 54^s{\cdot}8$ local mean time.

"The *Egress* was watched without the light neutral tint glass already mentioned. The images at *Ingress* were steady, but I think the definition at *Egress* was somewhat better. When the planet's preceding or western limb was within a second or so of reaching the Sun's limb, I noticed that the thread of light between the limbs almost suddenly became shaded by a ligament resembling that observed at Ingress, its breadth being also about one-fourth of the diameter of the planet. The formation of this ligament occurred at $3^h\ 53^m\ 0^s{\cdot}3$ local mean time. The observation was excellent, and the counting of the chronometer half-seconds was verified in the usual way. The Sun's limb was only faintly perceptible. Immediately following the above recorded time in the original notes of my observations are the entries $3^h\ 53^m\ 14^s{\cdot}8$ and $3^h\ 54^m\ 6^s{\cdot}3$ as corrected for the error of the chronometer. What these times represent I cannot now say, as the appropriate or corresponding phases are not stated, but it is probable that the first was an attempt to fix the instant of apparent coincidence of limb, and the second an attempt to estimate the time corresponding to some distance between the cusps of the Sun."

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Mr. CHARLES TODD, at the Adelaide Observatory, latitude 34° 55′ 33″ S., longitude $9^h\ 14^m\ 21^s.3$ E.,* observed with the 8-inch Refractor by T. Cooke & Sons, the aperture being reduced to 4 inches. The double [filar] micrometer eye-piece, with a [Ramsden] power of 140, was used throughout.

The Ingress was lost through cloud.

Egress.—"With regard to the 'black drop' of which so much has been written, I wish I had never heard of it, as looking eagerly for its appearance and not seeing what I expected was certainly calculated to bias my judgment as to the precise instant of internal contact. As the planet closed up to the Sun's limb it seemed to be but very slightly, perhaps a little, disturbed; but I saw nothing like the representations of the 'black drop.' There was, I thought, a slight oscillatory movement (of course only apparent) of the planet to and from the limb, which made it excessively difficult to fix positively the exact instant of contact, neither the Sun's limb nor the planet being sharply defined, but the time first noted by me—viz. $3^h\ 4^m\ 42^s.1$ local mean time—was when I thought the continuity of the Sun's limb, or the fine streak of light, appeared to be broken, although the planet did not appear to be quite in contact, but rather seemed to be connected with the Sun's limb by a most minute protuberance or fine filament, too small to be shown in any drawing. At $3^h\ 4^m\ 54^s.5$ the contact seemed to be full or tangential. Perhaps the true instant lies between the two times noted. I can hardly say to which I would attach the greater weight."

Mr. F. C. SINGLETON observed with a 3-inch Achromatic mounted in the Observatory grounds. Internal contact, $3^h\ 4^m\ 41$ Adelaide mean time.

Mr. J. D. SMEATON, J.P., observed at his residence, 136 chains north and 86 chains east of the Observatory, with an excellent Equatoreal of $3\frac{1}{2}$ inches by Cooke & Sons.

Internal contact at $3^h\ 5^m\ 5^s$ Adelaide mean time. No black drop, the osculation being perfect and the cusps of light between the Sun and planet quite sharp.

Mr. A. W. DOBBIE observed with an $8\frac{1}{2}$-inch silvered glass Reflector at his residence, about 880 yards north and 3·740 yards† east of the Observatory, his watch being compared with the transit clock at the Observatory. Internal contact at Egress, $3^h\ 5^m\ 6^s$ A.M.T.

Mr. Todd, who communicates these last three observations, adds: "I think it well to give you these outside observations, although, from the uncertainty in the determination of time, they may not be of great value."

* It is not stated on what the longitude depends.

† So in the original, not 3,740.

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Mr. D. FLITNER observed the *Ingress* at Waikiki, near Honolulu, with an Achromatic of $2\frac{1}{4}$ or $2\frac{1}{2}$ inches aperture, power 80–100. He recorded the "internal contact complete, and full sunlight between the planet and the Sun's limb," at $3^h\ 36^m\ 8^s{\cdot}4$ local mean time [$=7^h\ 18^m\ 4^s{\cdot}3$ Gr. sid. time]. Lat. 21° 16′ 0″ N., long. $10^h\ 31^m\ 19^s{\cdot}3$ W. (Tupman). His chronometer was compared with the Honolulu transit clock.

Don F. DIAZ COVARRUBIAS* observed at Nogue-no-yama, Japan, lat. 35° 26′ 54″·2 N., long. $9^h\ 18^m\ 34^s$ E., in the following manner:—A zenith telescope of $2\frac{3}{4}$ inches aperture, with its filar micrometer eye-piece, was made to produce an image of the Sun, $4\frac{3}{4}$ inches in diameter, upon a sheet of paper painted a light grey colour, the observer using a lens to view the image so produced.

"In regard to the internal contacts, the presence of the black ligament rendered it almost impossible to appreciate the exact instant of geometrical contact."

Ingress.

	h	m	s		
Internal geometrical contact	23	29	24·6	Local mean time.	
Rupture of the ligament	23	30	25·6	„	„

Egress.

	h	m	s		
Formation of the ligament	3	21	1·4	„	„
Internal geometrical contact	3	21	45·4	„	„

"I had hoped that this manner of observing would prevent the ligaments being seen; but, unfortunately, and notwithstanding the precision of the images, they were seen in a very marked manner. Not only were they seen, but the rupture at the first internal contact and the formation at the second, far from being instantaneous, took place by almost insensible gradations of the light, which necessarily caused some doubt as to the exact moments of disappearance and appearance. Equal or even greater uncertainty attended the estimation of the internal geometrical contacts, as is easily understood bearing in mind the extreme slowness with which the planet crossed the Sun's disk."

Don F. JIMENEZ, observing at Bluff, lat. 35° 26′ 16″·6 N., long. $9^h\ 18^m\ 39^s$ E., in precisely the same manner, the only difference being that his zenith telescope was $3\frac{3}{4}$ inches aperture, records:—

* *Observaciones del Tránsito de Vénus hechas en el Japon.* Paris, 1875. The longitude depends upon 14 Moon culminations.

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Ingress.

	h	m	s	
Internal geometrical contact	23	29	50·0	Local mean time.
Rupture of the ligament	23	30	43·5	,, ,,

Egress.

	h	m	s	
Formation of the ligament	3	21	20·9	Local mean time.
Internal geometrical contact	3	21	50·9	,, ,,

Discussion of the Observations.

The theory that the method of durations offers much greater precision in the determination of the solar parallax than that of the comparison of similar phases is founded entirely upon the supposition that the errors of observation of the times of contact are much less than the probable errors of the longitudes employed. No one will now deny that the reverse is the actual case. The errors of the longitudes here used can rarely amount to 5 seconds of time, and it is probable that nearly the whole of them are correct within 2^s or 3^s; whereas the difficulty of observing the internal contacts was such that observers side by side, with adequate optical means, differed as much as 20^s or 30^s in the times they recorded for phenomena which they have described in almost identical language. Errors of observation such as these affect the two methods equally when the parallactic conditions are equal.

It is evident, then, that the method of comparison of similar phases offers great advantages. It enables a very much larger number of observations to be employed, generally under more favourable parallactic conditions, and from the fact that the *Ingress* and *Egress* are entirely independent of each other, two strictly independent results are obtained, the agreement or disagreement of which will, in a great measure, influence the value to be attributed to them.

The parallax deducible from these observations depends, within certain not very narrow limits, upon the selection of the times corresponding to the phase under discussion. In many cases there is no choice—the observers have simply recorded "contact" at a definite instant. In others there will be no two opinions as to the proper times to select; while for the remainder, not a large proportion of the whole, the selection is a matter of judgment. The most important case open to dispute is that of the observations in the Sandwich Islands, where several observers recorded very late times, in my opinion merely to notify that all was over.

The Ingress.

The most satisfactory way perhaps to treat the *Ingress* is to rely upon the observation of the brightening or clearing of the

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dusky light seen by all observers between the limbs after the circular form of the Sun's limb was restored. With some of the smaller instruments, no doubt in some degree depending upon the colour and intensity of the absorbing media employed, this brightening occupied a very long time, but generally in the larger instruments the uncertainty was confined to less than 10ˢ. If a model, artificially representing as nearly as possible the circumstances of the transit, be set in the proper position for 10ˢ after internal contact at Ingress, a kind of shadow will be visible between the limbs when the attention is not strictly confined to the point of contact. This may perhaps explain some of the very late times recorded for Ingress complete, and the very early ones for beginning of Egress.

The following, then, are the times which I select, and the equations of condition founded on them, derived from the formulæ in the Astronomer Royal's Paper in the *Monthly Notices* for 1875, March. The symbol i is a constant for Ingress, common to all stations, compounded of the corrections required to the tabular semidiameters, the true distance between the limbs at the moment assumed as contact, and the effect of the imperfect corrections of the Tabular R.A. and N.P.D. These corrections have been assumed to be $+5''{\cdot}81$ and $-5''{\cdot}33$ respectively. The correction required to the tabular mean solar equatoreal horizontal parallax ($8''{\cdot}950$) is $8''{\cdot}950\left(1+\frac{n}{100}\right)$. The coefficient of dt is the variation of the distance of centres in one sidereal second, computed for the centre of the Earth, but sufficiently accurate for the surface.

Internal Contact at Ingress.

Observer.	Recorded Time. h	m	s	Local Tabular Distance of Centres. ′	″	Resulting Equation. ″	″	Weight.
Cator	1	20	29	15	37·47	$i+{\cdot}2284\,n+{\cdot}0343\,dt$	$=-{\cdot}388$	0
Noble	20	46	2	15	37·54	$i+{\cdot}2284\,n+{\cdot}0342\,dt$	$=-{\cdot}318$	1
Tupman	20	46	20	15	37·58	$i+{\cdot}2284\,n+{\cdot}0344\,dt$	$=-{\cdot}278$	2
Flitner	3	36	8·4	15	37·47	$i+{\cdot}2283\,n+{\cdot}0343\,dt$	$=-{\cdot}388$	1
Johnson	20	46	57·6	15	37·51	$i+{\cdot}2280\,n+{\cdot}0340\,dt$	$=-{\cdot}348$	2
Dunn	3	39	1·2	15	36·86	$i+{\cdot}2279\,n+{\cdot}0343\,dt$	$=-{\cdot}997$	0
Diaz	23	30	25·6	15	37·77	$i+{\cdot}1325\,n+{\cdot}0360\,dt$	$=-{\cdot}087$	1
Jimenez	23	30	43·5	15	37·28	$i+{\cdot}1328\,n+{\cdot}0352\,dt$	$=-{\cdot}576$	1
Palmer	18	58	52	15	38·075	$i-{\cdot}0142\,n+{\cdot}0318\,dt$	$=+{\cdot}205$	1
Russell	0	24	14	15	37·85	$i-{\cdot}0173\,n+{\cdot}0312\,dt$	$=-{\cdot}018$	2
Wright	0	24	30	15	37·32	$i-{\cdot}0172\,n+{\cdot}0315\,dt$	$=-{\cdot}542$	0
Lenehan	0	24	33	15	37·21	$i-{\cdot}0170\,n+{\cdot}0321\,dt$	$=-{\cdot}657$	0
Allerding	0	24	14	15	37·85	$i-{\cdot}0173\,n+{\cdot}0312\,dt$	$=-{\cdot}018$	0
Tebbutt	0	22	54·8	15	37·43	$i-{\cdot}0172\,n+{\cdot}0321\,dt$	$=-{\cdot}438$	2

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Observer.	Recorded Time. h	m	s	Local Tabular Distance of Centres. ′	″	Resulting Equation.	Weight.
Vessey	0	24	43·4	15	36·98	$i - ·0182\,n + ·0321\,dt = -·888$	0
Liversidge	0	25	1·5	15	36·97	$i - ·0238\,n + ·0305\,dt = -·896$	0
Tennant	12	42	30	15	37·25	$i - ·0295\,n + ·0324\,dt = -·609$	0
Scott	0	24	47·9	15	38·07	$i - ·0305\,n + ·0320\,dt = +·204$	2
McDonnell	0	25	0	15	37·68	$i - ·0305\,n + ·0305\,dt = +·012$	1
Gilbert	0	0	42·5	15	37·19	$i - ·0366\,n + ·0319\,dt = -·678$	0
Morris	0	0	45·8	15	37·08	$i - ·0366\,n + ·0319\,dt = -·788$	0
Bigg-Wither	12	17	8·	15	37·87	$i - ·0403\,n + ·0324\,dt = +·011$	1
Ellery	0	0	44·8	15	37·82	$i - ·0436\,n + ·0319\,dt = -·050$	2
White	0	0	44·0	15	37·84	$i - ·0436\,n + ·0319\,dt = -·030$	1
Wilson	0	0	39·5	15	38·10	$i - ·0448\,n + ·0319\,dt = +·230$	1
Neate	11	52	14·8	15	38·00	$i - ·1937\,n + ·0306\,dt = +·123$	2
Wharton	9	57	3	15	37·875	$i - ·1935\,n + ·0312\,dt = -·001$	1
Hoggan	6	36	32·8	15	37·35	$i - ·1935\,n + ·0296\,dt = -·526$	0
Corbet	12	20	8·	15	38·295	$i - ·2153\,n + ·0294\,dt = +·409$	2
Coke	8	43	46·5	15	37·81	$i - ·2152\,n + ·0287\,dt = -·076$	1
Goodridge	7	9	46·7	15	37·935	$i - ·2146\,n + ·0288\,dt = +·049$	1

The solution of these equations by the method of least squares, putting $dt = 0$ and giving them for the present equal weights, is

$$i = -0''·271$$

$$n = -0·975,$$

whence the mean solar parallax $= 8''·863$.

Substituting these values in the equations, we obtain the following values of dt in seconds of time, the sign + indicating that the observation as interpreted by me was too early :—

	s		s		s
Cator	+ 3·1	Lenehan	−12·6	Ellery	+ 5·6
Noble	+ 5·2	Allerding	+ 7·6	White	+ 6·2
Tupman	+ 6·3	Tebbutt	− 5·8	Wilson	+14·3
Flitner	+ 3·1	Vessey	−19·8	Neate	+ 6·7
Johnson	+ 4·3	Liversidge	−21·2	Wharton	+ 2·6
Dunn	−14·7	Tennant	−11·3	Hoggan	−15·0
Diaz	+ 8·7	Scott	+13·9	Corbet	+16·0
Jimenez	− 5·0	McDonnell	+ 8·3	Coke	− 0·5
Palmer	+14·5	Gilbert	−13·9	Goodridge	+ 3·9
Russell	+ 7·6	Morris	−17·3		
Wright	− 9·1	Bigg-Wither	+ 7·5		

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In order to derive the most satisfactory result, however, some system of weighting is indispensable. It is obviously absurd to give equal weights to observations made by experienced observers, with powerful optical means under favourable atmospheric conditions, and to those made under conditions more or less the reverse. Cator used a very low power, he gives no description of the phenomena observed, and his sketches were made afterwards from memory; Dunn is 20ˢ behind five other observers near him, his telescope was small and much shaken by the wind; Wright, Lenehan, and Allerding acknowledge their observations to be very unsatisfactory, they all waited much too long; Vessey, Liversidge, Gilbert, and Morris are between 20 and 30 seconds behind the average of eleven others in the same district; Tennant's time is very late, probably from the circumstance of the double-image micrometer eye-piece having been employed; and in Hoggan's case the subjective phenomena were exaggerated by the brightness of the image, which was insufficiently toned down.

I think these observations are not comparable with the mass of the others, and that their introduction slightly vitiates the result. I have therefore rejected them in the final discussion. Considering the optical means and the favourable atmospheric conditions, it seems fair to give the double weights shown in the column. There is a difference of 16ˢ between Corbet and Coke, who were side by side, and it is evident that Coke was late. I have given double weight to Corbet's observation.

There are now twenty observations left, eight of which have double weight, and the solution of the equations derived from them is:—

$$i = -1''.071$$
$$n = -0''.177,$$

whence the mean solar parallax is 8″·845.

Substituting, as before, we have the residuals:—

	s		s		s
Noble	+ 0·6	Russell	+ 1·1	Wilson	+ 7·8
Tupman	+ 1·9	Tebbutt	−12·1	Neate	− 1·1
Flitner	− 1·4	Scott	+ 7·5	Wharton	− 5·1
Johnson	− 2·4	McDonnell	+ 1·5	Corbet	+ 7·7
Diaz	+ 3·9	Bigg-Wither	+ 1·1	Coke	− 9·0
Jimenez	− 9·9	Ellery	− 1·0	Goodridge	− 4·6
Palmer	+ 8·1	White	− 0·9		

The errors of the rejected observations now appear to be:—

	s		s		s
Cator	− 1·4	Allerding	+ 1·1	Gilbert	−20·4
Dunn	−19·2	Vessey	−26·1	Morris	−23·8
Wright	−15·6	Liversidge	−27·9	Hoggan	−23·1
Lenehan	−18·9	Tennant	−17·7		

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The Egress.

It is very interesting, as illustrating the difficulty, or rather the unsatisfactory nature of the observation, and the small value that can be attributed to any recorded time, to compare the observations of Campbell, Tennant, Copeland, Gill, Russell, and Todd, on the one hand, with those of Ellery, Hunter, Mrs. Campbell, and Orde Browne, on the other. The former see little or nothing to interfere with an observation of tolerably definite contact; the latter have disturbing phenomena extending over various intervals, in Mr. Ellery's case to $2\frac{1}{2}$ *minutes* of time. Again, Captains Bigg-Wither and Strahan, at different stations, declare there was no contact at all; *Venus pushed the Sun's limb out before her.*

The following are the times which I consider should be selected for the critical phase of egress, and the equations deduced from them:—

Internal Contact at Egress.

Observer.	Recorded Time. h	m	s	Local Tabular Distance of Centres. ′	″	Resulting Equation.	Weight.
Wilson	3	29	31·4	15	42·01	$e - ·1313\,n - ·0328\,dt = + ·516$	1
Ellery	3	29	20·	15	41·49	$e - ·1297\,n - ·0317\,dt = - ·001$	2
White	3	29	15·	15	41·34	$e - ·1297\,n - ·0317\,dt = - ·149$	1
Liversidge	3	54	20·4	15	41·52	$e - ·1278\,n - ·0329\,dt = + ·015$	1
Tornaghi	3	54	25·8	15	41·69	$e - ·1278\,n - ·0329\,dt = + ·183$	1
Hixson	3	54	28·0	15	41·76	$e - ·1278\,n - ·0329\,dt = + ·252$	1
Russell	3	54	39·7	15	42·09	$e - ·1276\,n - ·0318\,dt = + ·573$	2
Lenehan	3	54	36·6	15	41·99	$e - ·1276\,n - ·0318\,dt = + ·473$	1
Wright	3	54	39·6	15	42·09	$e - ·1276\,n - ·0318\,dt = + ·573$	1
Allerding	3	54	35·	15	41·93	$e - ·1274\,n - ·0318\,dt = + ·415$	1
Moerlin	3	29	36 6	15	41·63	$e - ·1258\,n - ·0328\,dt = + ·130$	1
Tebbutt	3	53	0·3	15	41·67	$e - ·1259\,n - ·0330\,dt = + ·156$	2
Vessey	3	54	26·0	15	41·48	$e - ·1257\,n - ·0329\,dt = - ·031$	2
Todd	3	4	48·3	15	41·625	$e - ·1115\,n - ·0320\,dt = + ·103$	2
Singleton	3	4	41·	15	41·39	$e - ·1115\,n - ·0317\,dt = - ·122$	1
Smeaton	3	5	5·	15	42·18	$e - ·1118\,n - ·0331\,dt = + ·668$	0
Dobbie	3	5	6·	15	42·21	$e - ·1118\,n - ·0331\,dt = + ·698$	0
Goodridge	10	30	24·0	15	41·03	$e - ·1010\,n - ·0317\,dt = - ·346$	1
Sidgreaves	10	27	42·8	15	41·135	$e - ·1006\,n - ·0317\,dt = - ·243$	1
Perry	15	40	38·8	15	41·69	$e - ·1004\,n - ·0309\,dt = + ·306$	1
Finlay	19	7	11·0	15	41·63	$e - ·0590\,n - ·0314\,dt = + ·278$	1
Stone	19	7	12·0	15	41·66	$e - ·0590\,n - ·0314\,dt = + ·308$	2

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Observer.	Recorded Time. h	m	s	Local Tabular Distance of Centres. ′	″	Resulting Equation.	Weight.
Spindler	19	7	25·0	15	41·50	$e-\cdot0525\,n-\cdot0315\,dt = +\cdot031$	1
Hoggan	10	4	53·5	15	41·45	$e+\cdot0271\,n-\cdot0332\,dt = -\cdot086$	1
Neate	15	21	27·2	15	41·27	$e+\cdot0274\,n-\cdot0322\,dt = -\cdot263$	2
Wharton	1	25	48·1	15	41·62	$e+\cdot0275\,n-\cdot0320\,dt = +\cdot085$	1
Copeland	21	48	11·3	15	41·21	$e+\cdot0296\,n-\cdot0331\,dt = -\cdot313$	2
Gill	21	48	12·5	15	41·26	$e+\cdot0296\,n-\cdot0331\,dt = -\cdot264$	2
Diaz	3	21	1·4	15	41·39	$e+\cdot1265\,n-\cdot0352\,dt = -\cdot474$	1
Jimenez	3	21	20·9	15	41·93	$e+\cdot1260\,n-\cdot0350\,dt = +\cdot060$	1
Miss Pogson	23	24	32·	15	41·95	$e+\cdot1453\,n-\cdot0343\,dt = +\cdot191$	1
Chary	23	24	46·	15	42·43	$e+\cdot1453\,n-\cdot0343\,dt = +\cdot666$	0
Pillay	23	25	12·	15	43·31	$e+\cdot1452\,n-\cdot0353\,dt = +1\cdot535$	0
N. Row	16	47	15·4	15	38·48	$e+\cdot1579\,n-\cdot0340\,dt = -3\cdot255$	0
Addison	15	44	10·9	15	40·52	$e+\cdot1905\,n-\cdot0353\,dt = -1\cdot248$	0
Ommanney*	20	1	2·5	15	41·54	$e+\cdot1908\,n-\cdot0338\,dt = -\cdot166$	1
Mrs. Campbell*	6	14	57·5	15	41·55	$e+\cdot1908\,n-\cdot0338\,dt = -\cdot156$	1
Auwers*	18	57	58·	15	41·62	$e+\cdot1908\,n-\cdot0338\,dt = -\cdot087$	2
Col. Campbell*	6	6	31·	15	41·62	$e+\cdot1908\,n-\cdot0338\,dt = -\cdot087$	2
Döllen*	13	29	16·5	15	41·84	$e+\cdot1908\,n-\cdot0338\,dt = +\cdot133$	1
Tennant	16	28	36·1	15	41·55	$e+\cdot1959\,n-\cdot0364\,dt = -\cdot277$	2
Hennessey	18	5	32·6	15	40·83	$e+\cdot1973\,n-\cdot0350\,dt = -\cdot984$	0
Hunter	13	27	19·	15	41·61	$e+\cdot2021\,n-\cdot0340\,dt = -\cdot117$	2
Newton	13	22	20·7	15	41·34	$e+\cdot2016\,n-\cdot0339\,dt = -\cdot381$	1
Miss Newton	8	10	35·0	15	41·41	$e+\cdot2016\,n-\cdot0339\,dt = -\cdot312$	1
Browne	13	22	25·0	15	41·44	$e+\cdot2016\,n-\cdot0339\,dt = -\cdot278$	2
Mahmoud Bey	20	11	13·2	15	41·52	$e+\cdot2017\,n-\cdot0342\,dt = -\cdot204$	1
Strahan	16	14	36·9	15	41·30	$e+\cdot2026\,n-\cdot0365\,dt = -\cdot525$	1

I think it advisable to reject the observations of Smeaton and Dobbie, as no one guarantees the correctness of the local time. At Madras the Sun was barely visible through the clouds: perhaps none of the observations there should have been used. I have, however, retained the one which appeared to agree pretty well with the average. General Addison's observation being 30ˢ, Mr. Hennessey's 22ˢ, and Mr. Nursinga Row's a minute and a half earlier than the average, are also not included. None of them contain details by which the phase observed can be identified.

* The times of the Luxor observers have been taken from Dr. Auwers' *Bericht über die Beobachtung des Venus-Durchgangs vom* 8 *Dec.* 1874. Berlin, 1878.

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I have given double weight to some observers as at *Ingress*. The solution of these equations thus weighted is:

$$e = +0''.007$$
$$n = -1''.157,$$

whence the mean solar parallax is 8″·846.

The following are the residuals from this solution, the sign + indicating, as before, that the observation as interpreted by me was too early:—

	s		s		s
Wilson	−10·9	Singleton	+ 8·1	Miss Pogson	−10·3
Ellery	+ 5·0	Goodridge	+14·8	Ommanney	− 1·4
White	+ 9·7	Sidgreaves	+11·6	Mrs. Campbell	− 1·7
Liversidge	+ 4·3	Perry	− 5·9	Auwers	− 3·7
Tornaghi	− 0·9	Finlay	− 6·5	Campbell	− 3·8
Hixson	− 3·0	Stone	− 7·4	Döllen	−10·3
Russell	−13·1	Spindler	+ 1·1	Tennant	+ 1·6
Lenehan	−10·0	Hoggan	+ 1·9	Hunter	− 3·3
Wright	−13·1	Neate	+ 7·4	Newton	+ 4·6
Allerding	− 8·2	Wharton	− 4·7	Miss Newton	+ 2·5
Moerlin	+ 0·7	Copeland	+ 8·7	Browne	+ 1·5
Tebbutt	− 0·1	Gill	+ 7·2	MahmoudBey	− 0·7
Vessey	+ 5·6	Diaz	+ 9·5	Strahan	+ 8·2
Todd	+ 1·0	Jimenez	− 5·7		

The errors of the rejected observations appear to be:

	s		s		s
Smeaton	−16·1	Pillay	−48·0	Addison	+29·3
Dobbie	−17·0	N. Row	+90·6	Hennessey	+21·8
Chary	−24·0				

Looking at the residuals from this solution of the Egress, it is seen that the observations at Sydney, N. S. W., viz. by Russell, Lenehan, Wright, and Allerding, have great weight in lowering the value of the parallax, and this effect is aggravated by allowing Russell double weight, by Wilson's late observation at Mornington, and by the early observations of Diaz and Strahan at stations where the Egress was retarded. The extreme observations tending in the opposite direction, or to increase the parallax, have very far from an equal effect. These are the early times of White, Singleton, Goodridge, and Sidgreaves, and the late times of Miss Pogson and Döllen.

The adopted longitude of Sydney is probably not 3ˢ in error, and cannot have much to do with the late times observed there. Mr. Russell's is one of the most detailed observations made; it seems impossible, from his description, to choose his earlier time. Mr. Lenehan admits he was late, and I have taken 10ˢ from his

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time, but still it is late. Dr. Wright made "the most accurate observation possible," and agrees to a fraction of a second with Mr. Russell. Mr. Allerding does not profess to have made a good observation.

Captain Strahan's is an early observation by the wording, but so also are those of Moerlin, Stone, and Finlay, although every solution makes the two last named a few seconds *late*. Professor Wilson's is perhaps not an observation at all.

Out of mere curiosity I have solved the 35 equations that remain, when Wilson's, Strahan's, and those at Sydney are excluded, retaining the same weights as before, and find

$$\epsilon = -0''.059$$
$$\pi = -0''.626,$$

whence the mean solar parallax is 8″·894.

From this solution we have the following residuals:—

	s		s		s
Ellery	+ 0·7	Perry	− 9·8	Ommanney	− 0·4
White	+ 5·4	Finlay	− 9·6	Mrs. Campbell	− 0·7
Liversidge	+ 0·2	Stone	−10·5	Auwers	− 2·7
Tornaghi	− 4·9	Spindler	− 1·8	Campbell	− 2·7
Hixson	− 7·0	Hoggan	+ 0·3	Döllen	− 9·2
Moerlin	− 3·4	Neate	+ 5·8	Tennant	+ 2·6
Tebbutt	− 4·1	Wharton	− 5·0	Hunter	− 2·0
Vessey	+ 1·6	Copeland	+ 7·1	Newton	+ 5·8
Todd	− 2·9	Gill	+ 5·6	Miss Newton	+ 3·8
Singleton	+ 4·2	Diaz	+ 9·5	Browne	+ 2·8
Goodridge	+11·0	Jimenez	− 5·7	MahmoudBey	+ 0·6
Sidgreaves	+ 7·8	Miss Pogson	−10·0		

and the apparent errors of the others are:—

	s		s		s
Wilson	−15·0	Smeaton	−19·9	Addison	+30·3
Russell	−17·3	Dobbie	−20·8	Hennessey	+22·9
Lenehan	−14·2	Chary	−23·8	Strahan	+ 9·3
Wright	−17·3	Pillay	−47·7		
Allerding	−12·4	N. Row	+91·1		

This solution certainly satisfies very well a large majority of the observations, but it makes the Cape times appear 10ˢ and the Sydney 17ˢ late. Such a parallax as 8″·89 therefore appears improbable. On the other hand, if the extreme observations of Egress which tend to a large parallax be rejected—such as those of White, Goodridge, Sidgreaves, Miss Pogson, and Döllen, three of which were greatly interfered with, if not entirely spoiled, by clouds—a result as small as 8″·79 would be obtained.

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Although the above results of Ingress and Egress present such an unexpected agreement, it cannot be said that the mean 8″·8455 is entitled to much confidence, since all the observations would be fairly well satisfied by any mean solar parallax between 8″·82 and 8″·88. 8″·8455 corresponds to a mean distance of the Sun of 92,400,000 British statute miles.

Remarks on former Papers on the same Subject.

The Astronomer Royal's "Parliamentary Report," which was brought out somewhat hurriedly under pressure, contained the first discussion of detailed observations. As far as concerns the Ingress that report included all the more important observations—that is, those from which the Parallax is more directly determined. The treatment assumed that certain phases called α, β, γ, &c., separated some 20ˢ from one another, were recognised at every station, in spite of the very different language employed. The phases were all cast and criticised before any calculation of the parallax was made, and therefore the result was a perfectly legitimate deduction, not open to any suspicion of having been "doctored," which is more than can be said of later results. Mr. Stone has pointed out, and I have no doubt he is right, that the phases were incorrectly cast for the Sandwich Islands' observations, and consequently a systematic error was introduced. It is still a matter of opinion whether this is so. Mr. Christie and Mr. Gill have independently cast the phases, being guided only by the observers' language, and they arrive substantially at the cast adopted in the report, and therefore at a very small parallax (about 8″·74) from Ingress.

In the *Monthly Notices*, vol. xxxviii., p. 279, Mr. Stone has discussed these observations, disregarding the supposed phases, and depending upon the critical phase of "Ingress complete." He arrives at the parallax 8″·860±·04, giving all observers equal weight, including Cator and Hoggan. Mr. Stone considers that Corbet was too early, and favours the mean of Coke and Goodridge; but I must say I think Corbet's probably the best observation, and that the other two were late. Corbet's language is decisive that the shadow had disappeared at his last recorded time. The general tendency of the observation was to be late—everybody was late, but in different degrees.

The discussion of the Egress in the Parliamentary Report is perfectly satisfactory. The critical phase ζ gives a parallax of 8″·90. By allowing the earlier phases some weight the value of the parallax was reduced to 8″·85. Mr. Stone deduces the values 9″·04, 9″·06, and 8″·98±·08; but in view of the large number of observations of Egress now available, it is useless to discuss further the British observations *per se*.

In the *Monthly Notices*, vol. xxxviii., p. 11, the Astronomer Royal communicates a general solution of the equations of condition derived from the phases adopted in the Parliamentary Report. The parallax deduced is, of course, the same as that of the Report.

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The next paper appears as a portion of the Report of the Council published in February last (page 198). The former result of Ingress (8″·74) is accepted, and is combined with a new Egress result (8″·77) depending upon the phase ζ as supposed to be identified in all the available observations. Here again the attempt to fasten these phases, δ, ϵ, ζ, and η, as originally defined, upon everybody's observations led to very erroneous results. For instance, in the case of the Cape observations (as originally communicated) there was no alternative but to cast Mr. Stone's $19^h\ 7^m\ 12^s$ and Mr. Finlay's $19^h\ 7^m\ 11^s$ as δ. The language employed in describing the phase corresponding to the time is in both instances the very definition of δ, viz. "the first appearance of shadow between the limbs." δ appearing to be safely cast, ζ was found by taking the mean of Mr. Stone's two later times, which seemed supported by Mr. Maclear's only time, $19^h\ 7^m\ 48^s$; and thus a time for phase ζ was arrived at which was at least 36^s too late, as shown by subsequent calculation; but it was impossible to know this until the calculation had been made. As instances of how little the observer's language can be depended on in selecting similar phases, the earliest recorded times of Moerlin, Stone, Finlay, Vessey, and Tebbutt—and especially remarkable is the language of the two first named as indicating extremely early phases—are undoubtedly identical with the very last recorded times of Spindler, Mrs. Campbell, and Hunter, with late times of Ellery, White, and Browne, and also, very curiously, with the apparently very late time of Perry at Kerguelen's, who was 36^s later than Goodridge's first detection of shadow; 22^s later than the same observer's time for a black drop "as black as the planet"; and 18^s later than Sidgreaves' "circular contact."

In commencing an investigation of the parallax from these and similar observations, some selection of phases, depending on the observer's language and on the mutual agreement of observers not very far apart, must be made. Any selection of times made *after an investigation of the effects of parallax*, such as that I have now made, will always expose the result to the suspicion of having been "doctored."

The conclusion being inevitable that the numerous observations of the Transit of 1874, made with modern instruments of great power and excellence, only enable us to determine that the solar parallax probably lies between the values 8″·82 and 8″·88, we naturally suspect that the value obtained from comparatively few observations made more than one hundred years ago with very inferior means cannot, in the abstract, be deserving of much confidence. Indeed, Encke deduced the parallax 8″·60, and Stone 8″·91, from the observations of the Transit of 1769. The parallax obtained chiefly depends upon the observations of Green and Cook at Otaheite, who are very discordant in their recorded times; and we may be sure, from their descriptions of the appearance of the planet upon the Sun, that their instruments—reflectors of only 24 inches focal length, and unfortunately both alike—

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would nowadays be considered worthless. The probable error of their contacts, instead of being 3^s, as Mr. Stone thought nine or ten years ago, was more likely 20^s or 30^s. I consider that the observations of the Transit of 1769 are fairly satisfied, taking into account the instruments used and the errors actually committed in 1874, by any parallax between $8''{\cdot}60$ and $8''{\cdot}95$. The equations given by Mr. Stone (*Monthly Notices*, vol. xxviii., p. 263) enable anyone with great facility to find the effect of "doctoring" the times of duration.

On some Terms of Long Period in the Mean Motion of Mars.

By E. Neison.

I have lately discovered two new terms of long period in the mean motion of the planet *Mars* which have coefficients of sensible value, and whose existence will in part explain the present errors in Leverrier's Tables of *Mars*. These terms are due to the second power of the disturbing forces, and arise in the main from the action of one planet on the perturbations produced by another. Their value has been calculated by a very similar method to that employed by Leverrier in computing the value of the two terms of similar nature which he has included in his Tables of *Mars*. In general, however, it has been found advisable to use the perturbations of the co-ordinates rather than the perturbations of the elements. For, as a rule, if the former be employed the true value of the resulting coefficient will be given at once, whereas if the latter be used, the value will result from the addition of two large terms with contrary signs, each resulting from a distinct calculation. The values of the coefficients of the disturbing function, and of the perturbations of the elements or co-ordinates, have been derived from Leverrier's Memoirs in the *Annales de l'Observatoire de Paris*. Leverrier's notation is employed throughout.

The first of these terms is due to the combined action of the Earth and *Saturn* on the motion of *Mars*. It has the considerable period of 220·90 years, and the value of

$$
\begin{aligned}
&-0''{\cdot}3388 \sin ([2n''' - n'' - 2n^{v}]\, t + 2\epsilon''' - \epsilon'' - 2\epsilon^{v} + \varpi'') \\
&+0''{\cdot}0250 \sin (\quad\;\; " \qquad\quad " \quad + \varpi^{v}) \\
&+0''{\cdot}0248 \sin (\quad\;\; " \qquad\quad " \quad + \varpi'')
\end{aligned}
$$

Adding and reducing to numbers and it becomes

$$+0''{\cdot}3673 \sin (1^{\circ}{\cdot}630\, [Y - 1852{\cdot}08]).$$

The value of this term in the mean motion of the Earth is given in a short paper of mine in the *Monthly Notices* for

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November 1877. In that paper, my object being merely the perturbations in the motion of the Earth, and not in that of *Mars*, I contented myself by merely reversing the value for the principal term in the motion of the Earth, and thus obtained only a fraction of the complete value in the motion of *Mars*. The existence of this new term will appreciably affect the mean motion of *Mars* as determined by Leverrier, possibly by 0″·01, and in this case it would, at the present time, alter the position of *Mars* by 0″·25, in addition to the more direct action which the term itself will have.

The second term is still more important, for its effects cannot be eliminated by a change in the mean motion. It arises from the combined attraction of the Earth and *Jupiter* on the motion of *Mars*, and it has the comparatively short period of 46·52 years. It has the value of

$$\begin{aligned} &-0''\cdot2934 \sin ([6n''' - 3n'' - 2n^{iv}]\, t + 6\epsilon''' - 3\epsilon'' - 2\epsilon^{iv} - \varpi'''), \\ &+0''\cdot0530 \sin (\quad " \qquad " \qquad -\varpi''), \\ &+0''\cdot0025 \sin (\quad " \qquad " \qquad +\varpi^{iv} - 2\varpi'''). \end{aligned}$$

Uniting these separate portions, and reducing to numbers, the term becomes

$$0''\cdot3285 \sin (7^\circ\cdot740\,[Y - 1835\cdot75]).$$

This term will therefore change the place of *Mars* by two-thirds of a second in the short period of twenty years, so that in this term alone the place of *Mars* will be affected by this amount since the epoch of Leverrier's Tables. Further, it is probable it will still further alter Leverrier's value of the mean motion, for although its short period would prevent this in a long series of observations, where much weight is attached to the modern observations it will have some influence.

In considering the influence of these terms in the mean motion of *Mars*, it must not be forgotten that the values which have been given are in heliocentric co-ordinates. The first term can alter the geocentric longitude of *Mars* by nearly two seconds of arc, and the second term by more than one and a half second, and as these will systematically affect the observation for several years at a time, they will become very sensible quantities. Thus, from the combined effect of these two terms, the geocentric place of *Mars* during opposition thirty years hence will be four seconds greater than it was at the commencement of the century.

During my investigation I have come across a number of other terms of long period in the mean motion of *Mars* with coefficients of less magnitude than those which have been already mentioned, but with values sufficiently great to render them sensible in delicate observations of *Mars*. They are, in fact, more important than a great number of the terms which are included

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by Leverrier in his Tables, and as they might all be included in a single table of single entry, it appears to me that they might be included with advantage in the Tables of *Mars*.

These terms are the following, the epoch being 1800:—

$$-0''.0458 \sin\{[2n' - n - n^{IV}]\, t + 283^\circ\, 24\} \quad \text{or} \quad +0''.0458 \sin\{7^\circ.53\, [Y - 1836.60]\},$$

period 46·75 years. This term will augment the preceding term, and be confounded with it for a long period.

$$+0''.0420 \sin\{[4n''' - 2n'' - n^{IV}]\, t + 158^\circ\, 36'\} \quad \text{or} \quad +0''.0420 \sin\{15^\circ.28\, [Y - 1860.33]\},$$

period 23·37 years.

$$-0''.1024 \sin\{[4n''' - 2n' - 2n^{IV}]\, t + 206^\circ\, 48'\} \quad \text{or} \quad +0''.1024 \sin\{15^\circ.07\, [Y - 1861.48]\},$$

period 23·88 years. This term, with that preceding, will practically form a single term for many years, and this compound term will have a coefficient of 0″·15, and be capable of systematically altering the geocentric place of *Mars* by three-quarters of a second of arc in the very short period of 10 years. All these three, as well as the next term, arise from the combined action of the Earth and *Jupiter*.

$$+0''.0230 \sin\{[13n''' - 7n'' + n^{IV}]\, t + 334^\circ\} \quad \text{or} \quad -0''.0230 \sin\{1^\circ.368\, [Y - 1781]\},$$

period 262·2 years. Despite its long period this term is very small.

The next three terms arise from the combined action of *Jupiter* and *Saturn* on the planet *Mars*.

$$+0''.0243 \sin\{[n''' - 7n^{IV} + 2n^{V}]\, t + 314^\circ\, 43'\} \quad \text{or} \quad +0''.0243 \sin\{3^\circ.123\, [Y - 1814.50]\},$$

period 115·30 years.

$$+0''.0536 \sin\{[n''' - 8n^{IV} + 4n^{V}]\, t + 199^\circ\, 30'\} \quad \text{or} \quad +0''.0526 \sin\{2^\circ.505\, [Y - 1807.79]\},$$

period 143·70 years. Both these terms have small coefficients from the different portions neutralising each other.

$$+0''.1145 \sin\{[n''' - 10n^{IV} + 9n^{V}]\, t + 193^\circ\, 52'\} \quad \text{or} \quad +0''.1145 \sin\{2^\circ.100\, [Y - 1806.62]\},$$

period 171·60 years. This term, with that preceding, will unite for many years to form a compound term with a coefficient of 1″·7, and capable of sensibly altering the position of *Mars*. They will slightly affect the value of the mean motion determined by Leverrier.

The last term is due to the action of *Venus* and the Earth on *Mars*.

$$-0''.0900 \sin\{[8n''' - n' - 2n'']\, t + 52^\circ\, 1'\} \quad \text{or} \quad -0''.0900 \sin\{0^\circ.874\, [Y - 1740.52]\}.$$

It has a period of 411·7 years.

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The terms depending on the argument $[n'-4n'''+6n^{IV}]$ and $[n'''-9n^{IV}+7n^{V}]$ are insensible. The term arising from the combined action of the Earth and *Saturn* with the argument $[19n'''-10n''-3n^{IV}]$, or of the 6th order of the eccentricities, has the enormous period of 6,000 years, and it has a coefficient of over a second of arc. Its enormous period renders this insensible. It is remarkable, however, for it will be a secular term when the motions of the perihelion are considered. Apart from other considerations, it has been thought well to place the values of these terms on record for the purpose of saving useless labour to subsequent investigators.

Note on a Remarkable Property of the Analytical Expression for the Constant Term in the Reciprocal of the Moon's Radius Vector. By Professor J. C. Adams, M.A., F.R.S.

Let $nt+\varepsilon$ denote the mean longitude of the Moon at the time t; $n't+\varepsilon'$ that of the Sun.

$\xi = nt+\varepsilon-n't-\varepsilon'$, the mean elongation of the Moon from the Sun.

ϕ, the Moon's mean anomaly.

ϕ', that of the Sun.

η, the Moon's mean distance from the ascending node.

$c = \frac{d\phi}{ndt}$ and $g = \frac{d\eta}{ndt}$, so that $(1-c)n$ denotes the mean motion of the Moon's perigee, and $(g-1)n$ denotes the mean retrograde motion of the Moon's node, in a unit of time.

Also let e denote the mean eccentricity of the Moon's orbit.

e', the eccentricity of the Sun's orbit.

γ, the sine of half the mean inclination of the Moon's orbit to the ecliptic.

$m = \frac{n'}{n}$, the ratio of the mean motion of the Sun to that of the Moon.

μ, the sum of the masses of the Earth and Moon.

$a = \left(\frac{\mu}{n^2}\right)^{\frac{1}{3}}$, the mean distance in the purely elliptic orbit which the Moon if undisturbed would describe about the Earth in its actual periodic time.

To fix the ideas, we will suppose the quantities e and γ to be defined as in Delaunay's Theory of the Moon.

If r denote the Moon's radius vector, and if we omit terms depending on the Sun's parallax, then, as is well known, the value of $\frac{a}{r}$ may be expanded in an infinite series involving cosines of angles of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm 2k\eta,$$

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where i, j, j', k denote any positive integers, including zero, and the coefficient of the term with this argument contains $e^j e'^{j'} \gamma^{2k}$ as a factor, the remaining factor being a function of m, e^2, e'^2, and γ^2.

In particular, there is a constant term in $\frac{a}{r}$, corresponding to the case in which i, j, j', and k are all zero, and this term has the form

$$A + Be^2 + C\gamma^2 + Ee'^4 + 2Fe^2\gamma^2 + G\gamma'^4 + \&c.,$$

where

$$A = A_0 + A_1 e'^2 + A_2 e'^4 + \&c.$$
$$B = B_0 + B_1 e'^2 + B_2 e'^4 + \&c.$$
$$C = C_0 + C_1 e'^2 + C_2 e'^4 + \&c.$$
$$\&c. \quad \&c. \quad \&c.$$

and A_0, A_1 &c., B_0, B_1 &c., C_0, C_1 &c. are all functions of m.

Plana and, after him, Lubbock, Pontécoulant, and Delaunay have developed the functions of m which occur in the coefficients of the several terms of $\frac{a}{r}$ and of the other coordinates of the Moon, in series of ascending powers of m, and have severally determined, by different methods, the numerical coefficients of the leading terms in these developments.

With respect to the constant term in $\frac{a}{r}$, Plana showed that the quantities denoted above by B_0 and C_0, viz. the coefficients of e^2 and γ^2 in the above constant, both vanish when account is taken of the terms involving m^2 and m^3. Pontécoulant carried the development of the quantities B_0 and C_0 two orders higher, viz. to terms involving m^5, and found that these terms likewise vanish.

These investigations of Plana and Pontécoulant, however, while they show that the coefficients of the above mentioned powers of m vanish by the mutual destruction of the parts of which each of the coefficients is composed, supply no reason why this mutual destruction should take place, and throw no light whatever on the values of the succeeding coefficients in the series.

Thinking it probable that these cases in which the coefficients had been found to vanish were merely particular cases of some more general property, I was led to consider the subject from a new point of view, and on February 22, 1859, I succeeded in proving, not only that the coefficients B_0 and C_0 vanish identically, but that the same thing holds good of the more general coefficients B and C, so that the coefficients of

$$e^2, \ e^2e'^2, \ e^2e'^4, \ \&c.$$
$$\gamma^2, \ \gamma^2e'^2, \ \gamma^2e'^4, \ \&c.$$

in the constant term of $\frac{a}{r}$ are all identically equal to zero.

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Further reflection on the subject led me, several years later, to a simpler and more elegant proof of the property above mentioned.

This new proof was found on February 27, 1868, and I now venture to lay it before the Society. The resulting theorem is remarkable for a degree of simplicity and generality of which the lunar theory affords very few examples.

There are also two remarkable relations between the coefficients of e^4, $e^2\gamma^2$, and γ^4 in the constant term of $\frac{a}{r}$, which we before denoted by E, F, and G. These relations may be thus stated:

If the terms of the quantity c or $\frac{d\phi}{ndt}$ which involve e^2 and γ^2 be denoted by

$$He^2 + K\gamma^2,$$

and similarly if the terms of g or $\frac{d\eta}{ndt}$ which involve e^2 and γ^2 be denoted by

$$Me^2 + N\gamma^2,$$

where H, K, M, and N are functions of m and e'^2,
then we shall have

$$\frac{E}{F} = \frac{H}{K} \text{ and } \frac{F}{G} = \frac{M}{N}.$$

These relations are established by means of the same principle which was employed to prove the theorem above mentioned, viz. that $B = 0$ and $C = 0$.

They were, however, arrived at much later, namely on August 14, 1877.

Analysis.

Let x, y, z denote the rectangular coordinates of an imaginary Moon at any time t, the plane of xy being that of the ecliptic, and the axis of x the origin of longitudes.

Also let x', y' be the rectangular coordinates of the Sun, r' its radius vector, and μ' its mass.

Then if we neglect the terms which involve the Sun's parallax, the equations of motion are

$$\frac{d^2x}{dt^2} + \frac{\mu x}{r^3} + \frac{\mu' x}{r'^3} = \frac{3\mu' x'}{r'^5}(xx' + yy'),$$

$$\frac{d^2y}{dt^2} + \frac{\mu y}{r^3} + \frac{\mu' y}{r'^3} = \frac{3\mu' y'}{r'^5}(xx' + yy'),$$

$$\frac{d^2z}{dt^2} + \frac{\mu z}{r^3} + \frac{\mu' z}{r'^3} = 0.$$

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Now let x_1, y_1, z_1 be the rectangular coordinates, and r_1 the radius vector, of another imaginary Moon at the same time t as before, so that the same equations of motion hold good, and μ, μ', x', y', and r' are unaltered.

Hence

$$\frac{d^2x_1}{dt^2}+\frac{\mu x_1}{r_1^3}+\frac{\mu' x_1}{r'^3}=\frac{3\mu' x'}{r'^5}(x_1x'+y_1y'),$$

$$\frac{d^2y_1}{dt^2}+\frac{\mu y_1}{r_1^3}+\frac{\mu' y_1}{r'^3}=\frac{3\mu' y'}{r'^5}(x_1x'+y_1y'),$$

$$\frac{d^2z_1}{dt^2}+\frac{\mu z_1}{r_1^3}+\frac{\mu' z_1}{r'^3}=0.$$

Multiply the first set of equations by x_1, y_1, z_1 respectively, and subtract their sum from the sum of the similar equations in x_1, y_1, z_1 multiplied by x, y, z respectively.

Thus we find

$$\left(x\frac{d^2x_1}{dt^2}-x_1\frac{d^2x}{dt^2}\right)+\left(y\frac{d^2y_1}{dt^2}-y_1\frac{d^2y}{dt^2}\right)+\left(z\frac{d^2z_1}{dt^2}-z_1\frac{d^2z}{dt^2}\right)$$
$$+\mu(xx_1+yy_1+zz_1)\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right)=0;$$

or

$$\frac{d}{dt}\left(x\frac{dx_1}{dt}-x_1\frac{dx}{dt}\right)+\frac{d}{dt}\left(y\frac{dy_1}{dt}-y_1\frac{dy}{dt}\right)+\frac{d}{dt}\left(z\frac{dz_1}{dt}-z_1\frac{dz}{dt}\right)$$
$$+\mu(xx_1+yy_1+zz_1)\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right)=0.$$

Hence the quantity

$$(xx_1+yy_1+zz_1)\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right)$$

is a complete differential coefficient with respect to t, and therefore when developed in cosines of angles which increase proportionally to the time it cannot contain any constant term.*

Now

$$xx_1+yy_1+zz_1=\frac{1}{2}\{2rr_1+(r-r_1)^2-(x-x_1)^2-(y-y_1)^2-(z-z_1)^2\}$$

and

$$\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right)=\left(\frac{1}{r_1}-\frac{1}{r'}\right)\left\{\frac{3}{rr_1}+\left(\frac{1}{r_1}-\frac{1}{r'}\right)^2\right\}.$$

* We may remark here that neither of the quantities

$$(xx_1+yy_1)\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right),$$

$$zz_1\left(\frac{1}{r_1^3}-\frac{1}{r'^3}\right),$$

can contain any constant term, but no use is made of this in what follows.

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Hence, if $x-x_1$, $y-y_1$, $z-z_1$, and therefore also $r-r_1$, and $\frac{1}{r_1}-\frac{1}{r}$ be quantities of the first order with respect to any symbol, then

$$(xx_1+yy_1+zz_1)\left(\frac{1}{r_1^3}-\frac{1}{r^3}\right)$$

will differ from $3\left(\frac{1}{r_1}-\frac{1}{r}\right)$ by a quantity of the third order only.

Hence, in the case supposed, the quantity $\frac{1}{r_1}-\frac{1}{r}$ cannot contain any constant term of lower order than the third.

More generally, the constant part of $\frac{1}{r_1}-\frac{1}{r}$ cannot be of a lower order than the constant part of the product of the quantity $\frac{1}{r_1}-\frac{1}{r}$ multiplied by one or other of the quantities

$$\left(\frac{1}{r_1}-\frac{1}{r}\right)^2, \text{ or } (x-x_1)^2+(y-y_1)^2+(z-z_1)^2-(r-r_1)^2.$$

Now, as the two systems x, y, z and x_1, y_1, z_1 satisfy the same differential equations, the solutions can only differ from each other by involving different values of the arbitrary constants.

By applying the principle just stated to four different cases of variation of the arbitrary constants, we shall be able to prove the properties already enunciated, viz.

$$B = 0, \quad C = 0, \quad \frac{E}{F}=\frac{H}{K}, \quad \text{and} \quad \frac{F}{G}=\frac{M}{N}.$$

Let

$$x = u\cos(nt+\epsilon)-v\sin(nt+\epsilon),$$
$$y = u\sin(nt+\epsilon)+v\cos(nt+\epsilon),$$

and similarly

$$x_1 = u_1\cos(nt+\epsilon)-v_1\sin(nt+\epsilon),$$
$$y_1 = u_1\sin(nt+\epsilon)+v_1\cos(nt+\epsilon),$$

where $nt+\epsilon$ is supposed to retain the same value as before.

Then

$$(x-x_1)^2+(y-y_1)^2 = (u-u_1)^2+(v-v_1)^2.$$

Hence, in the statement of our principle, we may replace

$$(x-x_1)^2+(y-y_1)^2+(z-z_1)^2-(r-r_1)^2$$

by

$$(u-u_1)^2+(v-v_1)^2+(z-z_1)^2-(r-r_1)^2.$$

For the sake of simplicity, we will take the quantity which was before denoted by a as our unit of length, so that, instead of

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the quantity formerly designated by $\frac{a}{r}$, we shall write simply $\frac{1}{r}$.

Now it is known, *a priori*, that the values of r and u, as well as that of $\frac{1}{r}$, may be developed in an infinite series involving *cosines* of angles of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm 2k\eta,$$

where i, j, j', and k denote any positive integers whatever, including zero, and that the value of v may be developed in a similar series involving *sines* of the same angles.

Also we know that the coefficient of the term with the above argument occurring in any of these series contains $e^j e'^{j'} \gamma^{2k}$ as a factor, the remaining factor being a function of m, e^2, e'^2 and γ^2.

Similarly we know that the value of z may be developed in an infinite series involving *sines* of angles of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm (2k+1)\eta,$$

and that the coefficient of the term with this argument contains $e^j e'^{j'} \gamma^{2k+1}$ as a factor, the remaining factor being a function of m, e^2, e'^2 and γ^2, as in the former case.

It is essential to observe that $\frac{1}{r}$, r, u, and v involve only even powers of γ, while z involves only odd powers of the same quantity.

Having made these preliminary observations, we are now in a position to apply our principle to the four cases already alluded to.

Case I.

First, suppose that the values of x, y, z are those belonging to the solution in which e and γ vanish, therefore all the arguments in the values of $\frac{1}{r}$, r, u, and v will be of the form $2i\xi \pm j'\phi'$ and z will vanish.

Also let the values of x_1, y_1, z_1 belong to the solution in which e has a finite value, but γ is still $= 0$, while $nt+\varepsilon$, and therefore also n, retains the same value as before.

Hence z_1 also vanishes, and therefore $z - z_1 = 0$.

Then all the arguments which occur in the values of $\frac{1}{r}$, r, u, and v will also occur in those of $\frac{1}{r_1}$, r_1, u_1, and v_1, but the coefficients of the corresponding terms will differ by a quantity which contains e^2 as a factor.

Let the terms with these arguments be called terms of the *first class*.

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Also there will be additional terms in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, with arguments of the form

$$2i\xi \pm j\phi \pm j'\phi',$$

where j does not vanish, and the coefficients of these terms will all contain e as a factor.

Let the terms with these arguments be called terms of the *second class*.

Now, in the formation of the quantities

$$\left(\frac{1}{r_1}-\frac{1}{r}\right)^2 \text{ and } \left(\frac{1}{r_1}-\frac{1}{r}\right)\{(u-u_1)^2+(v-v_1)^2-(r-r_1)^2\}$$

terms with the argument zero can only arise by multiplying together three terms of the first class, one term of the first and two of the second class, or three terms of the second class, one of which at least involves e^2 as a factor. Such a term formed in the first of these ways would be of the order of e^6 at least, while one formed in the second or third of these ways would be of the order of e^4 at least. Hence, by the principle before proved, the value of $\frac{1}{r_1}-\frac{1}{r}$ can contain no constant term of the order of e^2.

Hence $B = 0$ generally, and as this holds good for every value of e', we must have

$$B_0 = 0,\ B_1 = 0,\ B_2 = 0,\ \&c.$$

Case II.

In the next place, let the values x, y, z, as before, belong to the solution in which e and γ vanish, and let the values x_1, y_1, z_1 belong to the solution in which e is still equal to 0, but γ has a finite value, while $nt+\epsilon$, and therefore also n, retains the same value as before.

Then all the arguments which occur in the values of $\frac{1}{r}$, r, u, and v likewise occur in those of $\frac{1}{r_1}$, r_1, u_1, and v_1, but the coefficients of the corresponding terms will differ by a quantity which contains γ^2 as a factor.

Also there will be additional terms in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, with arguments of the form

$$2i\xi \pm j'\phi' \pm 2k\eta,$$

where k does not vanish, and these will also contain γ^2 as a factor in every term.

Hence $\frac{1}{r_1}-\frac{1}{r}$, $r-r_1$, $u-u_1$, and $v-v_1$ will contain γ^2 as a factor in every term.

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Also $z = 0$, and therefore $(z-z_1)^2 = z_1^2$, which will also contain γ^2 as a factor in every term.

Hence $\left(\frac{1}{r_1}-\frac{1}{r}\right)^3$ will be of the order of γ^6 at least, while

$$\left(\frac{1}{r_1}-\frac{1}{r}\right)\{(y-u_1)^2+(v-v_1)^2+(z-z_1)^2-(r-r_1)^2\}$$

will be of the order of γ^4 at least.

Therefore, by the same principle as before, the value of $\frac{1}{r_1}-\frac{1}{r}$ can contain no constant term of the order of γ^2.

That is, $C = 0$ generally; and as this holds good for every value of e', we must have

$$C_0 = 0,\ C_1 = 0,\ C_2 = 0,\ \&c.$$

Case III.

Next, let the values x, y, z belong to the solution in which γ vanishes and e is finite, while x_1, y_1, z_1 belong to the general case in which e_1 and γ are both finite, the value of e being now changed to e_1 while $nt+\epsilon$, and therefore also n, retains the same value as before.

Then all the arguments which occur in the values of $\frac{1}{r}$, r, u, and v, and which are of the form

$$2i\xi \pm j\phi \pm j'\phi',$$

will occur unchanged in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, provided that ϕ, and therefore also $\frac{d\phi}{ndt}$ or c, remains unchanged, but the coefficients of the corresponding terms will differ by quantities which involve either $e-e_1$ or γ^2 as a factor.

Let the terms with these arguments be called terms of the *first class.*

Also there will be additional terms in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, the arguments of which are of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm 2k\eta,$$

where k does not vanish. The coefficients of these terms will all contain γ^2 as a factor.

Call the terms with these arguments terms of the *second class.*

And $(z-z_1)^2 = z_1^2$, which contains γ^2 as a factor in every term.

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Now the condition that c remains unchanged gives us the following relation between e^2, e_1^2, and γ^2:

$$He^2 = He_1^2 + K\gamma^2,$$

taking into account only the terms of lowest order in e^2, e_1^2, and γ^2.

Hence, ultimately,

$$\gamma^2 = \frac{H}{K}(e^2 - e_1^2).$$

If this value of γ^2 be substituted for it, we see that every term in the values of $\frac{1}{r_1} - \frac{1}{r}$, $r - r_1$, $u - u_1$, $v - v_1$, and $(z - z_1)^2$ will be divisible by $e - e_1$.

Hence the constant part of $\frac{1}{r_1} - \frac{1}{r}$ will be divisible by $(e - e_1)^2$, and therefore also by $(e^2 - e_1^2)^2$, since this constant part involves only even powers of e^2 and e_1^2.

That is,

$$E(e_1^4 - e^4) + 2Fe_1^2\gamma^2$$

is divisible by $(e^2 - e_1^2)^2$; or,

$$E(e_1^4 - e^4) + 2Fe_1^2\frac{H}{K}(e^2 - e_1^2)$$

is divisible by $(e^2 - e_1^2)^2$.

Divide by $e^2 - e_1^2$ and then put $e_1^2 = e^2$, therefore

$$-2Ee^2 + 2F\frac{H}{K}e^2 = 0,$$

or

$$\frac{E}{F} = \frac{H}{K}.$$

Case IV.

Lastly, let the values of x, y, z belong to the solution in which e vanishes and γ is finite, while x_1, y_1, z_1 belong to the general case in which e and γ_1 are both finite, the value of γ being changed to γ_1 while $nt + \epsilon$, and therefore also n, retains the same value as before.

Then all the arguments which occur in the values of $\frac{1}{r}$, r, u, and v, and which are of the form

$$2i\xi \pm j'\phi' \pm 2k\eta,$$

will occur unchanged in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, provided that η, and therefore also $\frac{d\eta}{ndt}$ or g, remains unchanged, but the coefficients of the corresponding terms will differ by quantities which involve either e^2 or $\gamma^2 - \gamma_1^2$ as a factor.

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Let the terms with these arguments be called terms of the *first class.*

Also there will be additional terms in the values of $\frac{1}{r_1}$, r_1, u_1, and v_1, the arguments of which are of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm 2k\eta,$$

where j does not vanish. The coefficients of these terms will all involve e as a factor.

Call the terms with these arguments terms of the *second class.*

Moreover, all the arguments which occur in the value of z, and which are of the form

$$2i\xi \pm j'\phi' \pm (2k+1)\,\eta,$$

will occur unchanged in the value of z_1, but the coefficients of the corresponding terms will differ by quantities which involve either e^2 or $\gamma-\gamma_1$ as a factor.

Let the terms with these arguments be called terms of the *first class.*

Also there will be additional terms in the value of z_1, the arguments of which are of the form

$$2i\xi \pm j\phi \pm j'\phi' \pm (2k+1)\,\eta,$$

where j does not vanish. The coefficients of these terms will all involve $e\gamma_1$ as a factor.

Call the terms with these arguments terms of the *second class.*

Now the condition that g remains unchanged gives us the following relation between e^2, γ^2, and ${\gamma_1}^2$:

$$N\gamma^2 = Me^2 + N{\gamma_1}^2,$$

taking into account only the terms of lowest order in e^2, γ^2, and ${\gamma_1}^2$.

Hence, ultimately,

$$e^2 = \frac{N}{M}(\gamma^2-{\gamma_1}^2).$$

If this value of e^2 be substituted for it, we see that every term of the first class in the values of

$$\frac{1}{r_1}-\frac{1}{r},\quad r-r_1,\quad u-u_1,\quad \text{and}\quad v-v_1$$

will be divisible by $\gamma^2-{\gamma_1}^2$, and that every term of the second class in the values of the same quantities will be divisible by e. Also every term of the first class in the value of $z-z_1$ will be divisible by $\gamma-\gamma_1$, and every term of the second class in the value of the same quantity will be divisible by $e\gamma_1$.

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Now in the formation of the quantities

$$\left(\frac{1}{r_1}-\frac{1}{r}\right)^2,\quad \left(\frac{1}{r_1}-\frac{1}{r}\right)\{(u-u_1)^2+(v-v_1)^2-(r-r_1)^2\},\quad \text{and}\quad \left(\frac{1}{r_1}-\frac{1}{r}\right)(z-z_1)^2,$$

terms with the argument zero can only arise by multiplying together either

(1) Three terms of the first class;

(2) One term of the first and two of the second class;

or (3) Three terms of the second class, one of which at least involves e^2 as a factor.

Such a term formed in the first of these ways would be divisible by $(\gamma-\gamma_1)^3$ and therefore by $(\gamma^2-\gamma_1^2)^3$, since it can only involve even powers of γ and γ_1.

Such a term formed in the second of these ways would be divisible by $e^2(\gamma-\gamma_1)$ and therefore by $e^2(\gamma^2-\gamma_1^2)$ or by $(\gamma^2-\gamma_1^2)^2$.

Also such a term formed in the third of these ways would be divisible by e^4 or by $(\gamma^2-\gamma_1^2)^2$.

Hence, by the same principle as before, the value of $\frac{1}{r_1}-\frac{1}{r}$ must be divisible by $(\gamma^2-\gamma_1^2)^2$.

That is

$$2Fe^2\gamma_1^2+G(\gamma_1^4-\gamma^4)$$

is divisible by $(\gamma^2-\gamma_1^2)^2$; or

$$2F\frac{N}{M}(\gamma^2-\gamma_1^2)\gamma_1^2-G(\gamma^4-\gamma_1^4)$$

is divisible by $(\gamma^2-\gamma_1^2)^2$.

Now divide by $\gamma^2-\gamma_1^2$, and then put $\gamma_1^2=\gamma^2$; therefore

$$2F\frac{N}{M}\gamma^2-2G\gamma^2=0,$$

or

$$\frac{F}{G}=\frac{M}{N},$$

which is the last of the relations announced above.

The results obtained in Cases III. and IV. may be rendered more general in the following manner:—

Let P denote the constant term in the reciprocal of the Moon's radius vector, considered as a function of e^2 and γ^2.

Then, taking e^2, e_1^2, and γ^2 to be related as in Case III., we have, by the same reasoning as before,

$$0=\frac{dP}{d(e^2)}(e_1^2-e^2)+\frac{dP}{d(\gamma^2)}\cdot\gamma^2+\text{terms of higher dimensions in } e_1^2-e^2 \text{ and } \gamma^2.$$

Also

$$0=\frac{dc}{d(e^2)}(e_1^2-e^2)+\frac{dc}{d(\gamma^2)}\cdot\gamma^2+\text{terms of higher dimensions in } e_1^2-e^2 \text{ and } \gamma^2.$$

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Hence, we have ultimately, when $e_1^2 = e^2$, and $\gamma^2 = 0$,

$$\text{Limit of } \frac{\gamma^2}{e^2 - e_1^2} = \frac{\dfrac{dP}{d(e^2)}}{\dfrac{dP}{d(\gamma^2)}} = \frac{\dfrac{dc}{d(e^2)}}{\dfrac{dc}{d(\gamma^2)}},$$

in which γ^2 is to be put $= 0$ after the differentiations. The relation thus deduced holds good for all values of e^2. By equating the coefficients of e^2 on the two sides of the equation

$$\frac{dP}{d(e^2)} \cdot \frac{dc}{d(\gamma^2)} = \frac{dP}{d(\gamma^2)} \cdot \frac{dc}{d(e^2)},$$

we find $\dfrac{E}{F} = \dfrac{H}{K}$, as before.

Also, by equating the coefficients of higher powers of e^2, we obtain other relations between the coefficients of terms of higher orders in the value of P.

Similarly, taking e^2, γ^2, and γ_1^2 to be related as in Case IV., we have, by the same reasoning as before,

$$0 = \frac{dP}{d(e^2)} \cdot e^2 + \frac{dP}{d(\gamma^2)} (\gamma_1^2 - \gamma^2) + \text{terms of higher dimensions in } e^2 \text{ and } \gamma_1^2 - \gamma^2.$$

Also

$$0 = \frac{dq}{d(e^2)} \cdot e^2 + \frac{dq}{d(\gamma^2)} (\gamma_1^2 - \gamma^2) + \text{terms of higher dimensions in } e^2 \text{ and } \gamma_1^2 - \gamma^2$$

Hence, we have ultimately, when $e^2 = 0$ and $\gamma_1^2 = \gamma^2$,

$$\text{Limit of } \frac{\gamma^2 - \gamma_1^2}{e^2} = \frac{\dfrac{dP}{d(e^2)}}{\dfrac{dP}{d(\gamma^2)}} = \frac{\dfrac{dq}{d(e^2)}}{\dfrac{dq}{d(\gamma^2)}},$$

in which e^2 is to be put $= 0$ after the differentiations. The result thus deduced holds good for all values of γ^2. By equating the coefficients of γ^2 on the two sides of the equation

$$\frac{d(P)}{d(e^2)} \cdot \frac{dq}{d(\gamma^2)} = \frac{dP}{d(\gamma^2)} \cdot \frac{dq}{d(e^2)},$$

we find $\dfrac{F}{G} = \dfrac{M}{N}$, as before.

Similarly, by equating the coefficients of higher powers of γ^2, we obtain other relations between the coefficients of terms of higher orders in the value of P.

It may not be without interest to give here the result which I have obtained for the development of the constant term in the reciprocal of the Moon's radius vector.

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The expression includes, besides the terms spoken of in the foregoing paper, an additional term depending on the square of the Sun's parallax. Reintroducing the symbol a to denote the length before defined, which in the paper has been taken as the unit of length, I find

The constant term in $\frac{a}{r}$

$$= 1 + \frac{1}{6}m^2 - \frac{179}{288}m^4 - \frac{97}{48}m^5 - \frac{757}{162}m^6 - \frac{4039}{432}m^7 - \frac{34751189}{1990656}m^8 - \frac{31013527}{995328}m^9$$
$$+ e'^2\left[\frac{1}{4}m^2 - \frac{799}{192}m^4 - \frac{873}{32}m^5 - \frac{287849}{2304}m^6 - \frac{268607}{576}m^7\right]$$
$$+ e'^4\left[\frac{5}{16}m^2 - \frac{5401}{384}m^4 - \frac{18527}{128}m^5\right]$$
$$+ \frac{a^2}{a'^2}\left[\frac{3}{16}m^2 + \frac{75}{128}m^3\right]$$
$$+ e^4\left[\frac{1}{16}m^2 + \frac{225}{128}m^3\right]$$
$$+ e^2\gamma^2\left[2m^2 + \frac{63}{8}m^3\right]$$
$$+ \gamma^4\left[-m^2 + \frac{9}{8}m^3\right],$$

where e and γ have the same significations as in Delaunay's Theory.

The method which I employed in obtaining this expression is closely related to my first method, above alluded to, of proving the evanescence of the coefficients B and C.

The coefficients of e^4 and γ^4 were found independently, and from each of these, by means of the relations proved above, was derived a value of the coefficient of $e^2\gamma^2$. The perfect coincidence of these values supplied a test of the correctness of the calculations.

The terms of c and g which are required for this verification are the following:

$$c = \ldots + e^2\left(\frac{3}{8}m^2 + \frac{675}{64}m^3\right) + \gamma^2\left(6m^2 + \frac{189}{8}m^3\right) + \ldots$$

$$g = \ldots + e^2\left(\frac{3}{2}m^2 + \frac{189}{32}m^3\right) - \gamma^2\left(\frac{3}{2}m^2 - \frac{27}{16}m^3\right) + \ldots$$

I hope to lay the details of these calculations before the Society on some future occasion.

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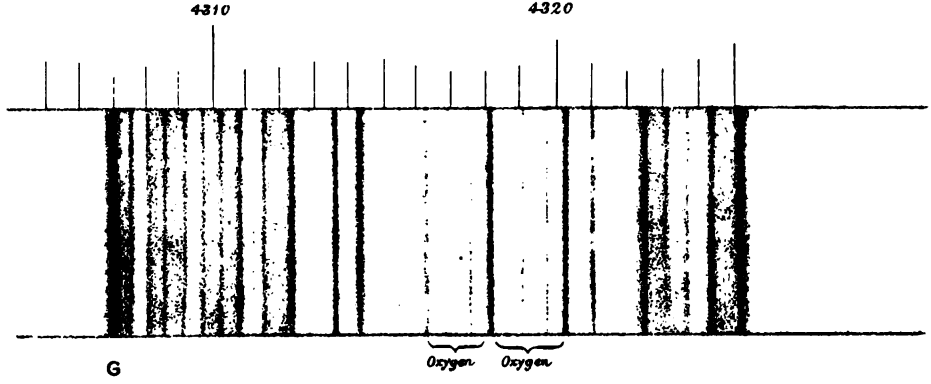
ROYAL OBSERVATORY GREENWICH.

SOLAR SPECTRUM NEAR G AS SEEN WITH HALF-PRISM SPECTROSCOPE.



PROF. DRAPER'S BRIGHT LINES.

Spottiswoode & Co. Lith. London.

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On the Existence of Bright Lines in the Solar Spectrum.

By W. H. M. Christie, Esq.

Since Professor H. Draper's announcement that bright lines corresponding to those of oxygen were to be detected in the solar spectrum, I have repeatedly examined various parts of the spectrum, and more particularly the neighbourhood of the G line, in the hope of being able to verify this important announcement. The result, however, has been that certain difficulties as regards the existence of bright lines have presented themselves, and I think it desirable to state these without further delay, although I have not been able to devote as much time to the inquiry as I could have wished. The lines in the solar spectrum present such a different appearance, according to the power of the spectroscope used, that a description of the aspect of the spectrum in the neighbourhood of the G line as seen with the half-prism spectroscope of the Royal Observatory, Greenwich, may throw some light on the question. With this spectroscope many fine lines, not shown in Ångström's or Kirchhoff's maps, or in Professor Draper's photograph, are readily seen, and the stronger absorption lines are comparatively narrow and sharply defined. As a consequence of this a space between two dark lines which, with a spectroscope of lower power, looks like a bright line, loses this appearance altogether, and seems to be nothing but the background of continuous spectrum. In the diagram representing the spectrum on the less refrangible side of G, four spaces are formed by the strong lines at 4314·4, 4316·3, 4318·1, and 4320·2, which, with a lower dispersive power or less perfect definition, might be taken as bright lines, and, in fact, Professor H. Draper has identified the two inner as a double line of oxygen. But, as seen at Greenwich, the spaces are each about ten times the breadth of the dark lines and of perfectly even tint, without any trace of fuzziness at the edges. Now it seems difficult to explain the existence of bright lines which remain of sensible breadth and sharply defined at the edges when the slit of the spectroscope is closed. In ordinary cases, where a bright line (such as H_β at atmospheric pressure) has a breadth much greater than that of the slit, it is fuzzy at the edges, whereas the spaces or 'bright lines' in question are perfectly even in tint. Further, I have not been able to distinguish the slightest difference in tint in the whole portion from 4312 to 4322, and this under conditions which showed, in each of the spaces, two fine absorption lines of which not a trace is to be found on Professor Draper's photograph. In making this statement, however, it must be understood that I am referring to the Albertype print, in which, doubtless, some of the definition of the original negative would be lost. I have not had an opportunity of seeing the latter. However this may be, the definition, when these observations

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with the half-prism spectroscope were made, was sufficiently good to show ten absorption lines in a space where Angström's map shows only three. The circumstance that there are absorption lines in these spaces seems to me to present another difficulty in accepting the view that the spaces in question are bright lines.

As Professor H. Draper attaches much importance to the evidence of photography, I have also taken a few photographs of this part of the spectrum, in which the relative breadths of the absorption lines, as compared with the spaces between them, and the fine lines seen in these spaces agree exactly with the eye-view. These photographs, which I beg to lay before the meeting, were taken with exposures ranging from 1ˢ to 5ˢ (according to the state of the sky) the width of the slit being about 1-5000th of an inch. But hitherto no comparison has been made at Greenwich with the spectrum of oxygen, my attention having been directed entirely to the character of the solar spectrum apart from the question of coincidences of spaces with the bright lines of oxygen. The comparison of the two spectra presents great practical difficulties, and the character of the solar spectrum is, after all, the question of primary importance. It might seem, at first sight, that the coincidence of a large number of lines of oxygen with spaces in the solar spectrum would be sufficient in itself to prove that these spaces are really bright lines, but it must be remembered that, as there are no coincidences of oxygen with dark lines in the solar spectrum, every line of oxygen must correspond approximately to a space, and this is all that can be asserted, so far, from the Albertype print of Professor Draper's photograph, in which the dark lines and spaces are of about equal breadth and fuzzy at the edges. If, however, an *exact* coincidence could be established within small limits of error on the original negative, the case would be altered. But the spectrum must be magnified many times to give the requisite accuracy. In this connexion it may be well to mention that the diagram is on a scale fifteen times that of Professor Draper's photograph and represents fairly the accuracy attainable in eye observation.

The observations were made with the half-prism spectroscope, in which a train of two compound half-prisms (direct vision) gives a dispersion of 75° from A to H, equivalent to 15 prisms of 60°. The spectroscope was used in connexion with the Great Equatoreal of the Royal Observatory, which forms an image of the Sun two inches in diameter on the slit, thus avoiding the confusion of definition which, as Capt. Abney has pointed out, would, to a certain extent, result from the effect of the Sun's rotation when a heliostat is used. In this examination of the spectrum I have received much assistance from Mr. Maunder.

1878, *June* 13.

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Ephemeris for finding the position of the Satellite of Neptune.

By Mr. A. Marth.

Gr. Noon. 1878.		P °	a "	b "	log. a	log. b	Long. °	Diff. °
Aug.	13	311·71	16·53	5·87	1·2182	0·7690	245·64	
								612·59
	23	311·69	16·62	5·91	·2206	·7713	138·23	
								·54
Sept.	2	311·65	16·70	5·93	·2228	·7729	30·77	
								·49
	12	311·58	16·78	5·94	·2248	·7737	283·26	
								·44
	22	311·48	16·85	5·94	·2266	·7738	175·70	
								·40
Oct.	2	311·36	16·90	5·93	1·2280	0·7731	68·10	
								612·36
	12	311·22	16·95	5·91	·2291	·7717	320·46	
								·33
	22	311·08	16·97	5·88	·2297	·7696	212·79	
								·31
Nov.	1	310·93	16·98	5·85	·2299	·7669	105·10	
								·31
	11	310·79	16·97	5·80	·2296	·7636	357·41	
								·31
	21	310·65	16·94	5·75	·2288	·7600	249·72	
								·32
Dec.	1	310·52	16·89	5·70	1·2276	0·7561	142·04	
								612·34
	11	310·41	16·83	5·65	·2261	·7521	34·38	
								·37
	21	310·33	16·76	5·60	·2242	·7482	286·75	
								·41
	31	310·27	16·67	5·55	·2220	·7445	179·16	
								·46
1879. Jan.	10	310·24	16·58	5·51	·2196	·7412	71·62	
								·50
	20	310·23	16·49	5·48	·2171	·7385	324·12	
								·55
	30	310·25	16·39	5·45	·2146	·7363	216·67	
								612·61
Feb.	9	310·31	16·30	5·43	1·2121	0·7347	109·28	

P, angle of position of the minor axis of the apparent orbit in the direction of superior conjunction.

a, *b*, major and minor semi-axes of apparent orbit.

Long. = longitude of the satellite in its orbit, reckoned from the point which is in superior conjunction with the planet.

The values of the table are to be interpolated for the times for which the places of the satellite are required, and the position-angles p and distances s are then found by

$$s \sin (P-p) = a \sin \text{long.}$$

$$s \cos (P-p) = b \cos \text{long.}$$

To know whereabout the satellite is situated it will be sufficient

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to know the approximate times of its superior ($p = 311°$) and inferior ($p = 131°$) conjunctions, and to construct the apparent ellipse.

1878.	G.M.T. h		1878.	G.M.T. h		1878.	G.M.T. h	
Aug. 14	21	sup.	Oct. 15	14	inf.	Dec. 16	8	sup.
17	19	inf.	18	13	sup.	19	6	inf.
20	18	sup.	21	11	inf.	22	5	sup.
23	16	inf.	24	10	sup.	25	3	inf.
26	15	sup.	27	8	inf.	28	2	sup.
29	13	inf.	30	7	sup.	31	0	inf.
Sept. 1	12	sup.	Nov. 2	5	inf.	1879.		
4	10	inf.	5	4	sup.	Jan. 2	23	sup.
7	9	sup.	8	2	inf.	5	21	inf.
10	8	inf.	11	1	sup.	8	20	sup.
13	6	sup.	14	0	inf.	11	18	inf.
16	5	inf.	16	22	sup.	14	17	sup.
19	3	sup.	19	21	inf.	17	16	inf.
22	2	inf.	22	19	sup.	20	14	sup.
25	0	sup.	25	18	inf.	23	13	inf.
27	23	inf.	28	16	sup.	26	11	sup.
30	21	sup.	Dec. 1	15	inf.	29	10	inf.
Oct. 3	20	inf.	4	13	sup.	Feb. 1	8	sup.
6	18	sup.	7	12	inf.	4	7	inf.
9	17	inf.	10	11	sup.	7	5	sup.
12	15	sup.	13	9	inf.	10	4	inf.

Note on the Duplicity of the Companion to Rigel.

By S. W. Burnham, Esq.

More than six years ago, when using a power of 400 on my 6-inch Clark Refractor, I noticed what appeared to be a slight but decided elongation in the companion to *Rigel*, nearly in the direction of the primary. I called the attention of observers to this (*English Mechanic*, February 9, 1872), and subsequently examined the star many times with the same instrument, but was never able to satisfy myself fully that the small star was really double, and the evidence from other quarters was either negative or unfavourable. Some years later Mr. Herbert Sadler wrote me that he had noticed the same peculiar appearance in the small star, independently, I believe, and I again, in 1876,

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examined it on several very favourable occasions, finding the same appearance as before. I could not feel quite certain that it was not due to vertical elongation, although, after the precautions observed, it seemed very improbable. The attention of several astronomers in this country and Europe having larger apertures at command was specially requested to aid in deciding the question. Nothing having been received changing or modifying my own prior observations, at the first opportunity, towards the end of 1877, the 18½-inch Clark Equatoreal of the Dearborn Observatory was brought to bear, and the small star carefully examined whenever the air was sufficiently steady. There were few nights, however, at this time perfect enough to allow of the use of very high powers at a moderate altitude. Later the conditions were better, and I give below the measures when the disk of the small star was sharply enough defined to make it worth while attempting to use the micrometer. The star was examined on a number of other nights earlier, but generally the images were too unsteady to give anything more than a suspected, or uncertain elongation. But on no occasion, at any time, was the appearance such as to discourage further attempts.

February 3. Measured P = 160°·3. There does not seem to be much doubt of the duplicity of the small star. Power 900.

„ 26. Measured as follows: P = 179°·1; D = 0″·31. B is certainly double. Splendid definition. There can be no doubt now. Power 900.

March 4. Measures good. P = 178°·9; D = 0″·40. Companion is certainly double. There can be no mistake in the slight elongation measured, but excessively difficult. Measured distance is a diameter. Power 900.

It should be stated that the distance measures given above more properly belong to the long diameter of the small star than to the central distance, as otherwise the stars would, with this power, be widely separated. The real distance cannot exceed 0″·2, and is probably materially less. The position-angle measures were made with the large star out of the field, so that its direction would not bias the setting of the wires on the close pair. Several attempts were made later, but the air was not steady enough to add anything of value to the results already obtained, and no measures could be made. It may seem strange that a star so excessively close as this should have been even suspected with an aperture of only six inches, and it is not easy of explanation. I can only state that under suitable conditions single stars of this magnitude are seen with perfectly round, sharply cut, and well-defined disks, and any eccentricity, however small, might, with careful attention, be detected. If the distance were not under 0″·3 there would not be a moment's hesitation in pronouncing such a star double. I trust astronomers having sufficiently powerful and perfect instruments will give the star

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special attention. It is a severe test, and a strictly first-class telescope, whether large or small, is necessary to deal with it.

Rigel has a minute distant attendant, discovered by Mitchel at Cincinnati in 1846, and rated by him 15 magnitude. I made two measures as follows:—

	°	″
October 24	P = 1·7	D = 44·38.
„ 31	1·3	44·27.

This star has never been measured before.

Chicago, April 12.

New Double Stars. By S. W. Burnham, Esq.

I desire to call the attention of double star observers, in advance of the publication of my observations during the past year, to a few of the recent discoveries with the 18½-inch refractor of the Dearborn Observatory, in the hope that other measures may be made during the present season. The prevailing unfavourable weather here for several months has made it impossible to repeat the measures of some of the stars as often as was desired.

		°	″
1	51 Cygni	P = 102·3	D = 3·13
2	8 Andromedæ	162·0	7·40
3	27 Piscium	264·9	1·44
4	52 Herculis	309·4	1·84
5	15 Can. Ven.	284·9	1·22
6	ι′ Libræ, BC	24·3	1·86
7	Serpentis 55	356·6	0·42
8	B.A.C. 4559	57·0	0·26
9	γ Draconis	152·3	20·61

(5) 15 *Can. Ven.* is the preceding star of the wide pair, Σ 24, App. I.

(6) As a wide pair, this is ℍ VI. 44 = Sh. 376. Herschel's companion is double, the system somewhat resembling *μ Herculis*. There appears to be a decided change in the distance from the large star. Sh. found 111°·6 : 50″·3. From three nights' measures I get 110°·5 : 57″·46.

(7) The place of this star is R.A. $15^h\ 37^m$; Decl. + 14° 3′.

(8) This is a large star in *Boötes* (5 magnitude, according to Argelander and Heis), the place of which is R.A. $13^h\ 33^m$; Decl. + 11° 21′. This is perhaps the only really difficult pair in the list, with a large aperture. The distance given above is not too small.

(9) The attendant is very minute. There are two other stars of about the same magnitude, 227°·1 : 47″·73 and 13°·7 : 56″·48. These should not be confounded with brighter and more distant stars in the same quadrants.

The other pairs are all unequal, but can be observed with moderate apertures.

Chicago, May 25.

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On the Position of λ Ursæ Minoris.

By Professor T. H. Safford.

The star in question is extremely well known; but its proper motion and nearness to the Pole make it very troublesome to predict its place; so that it furnishes a good example of the treatment of proper motions in that region of the heavens.

I begin with the assumption of the following formula, as representing its position for time t referred to the Equator and Equinox of 1855:—

$$\text{A.R.} = 20^{h}\ 8^{m}\ 30^{s}\cdot200 - 0^{s}\cdot073\ (t-1855),$$

$$\text{Decl.} = 88^{\circ}\ 52'\ 32''\cdot30 + 0''\cdot01\ (t-1855).$$

These were derived from a first approximate solution, after reducing to 1855 the material most conveniently at hand.

The first step is to calculate for various epochs the positions which correspond to this formula, after application of precession to the epochs in question.

With the position for 1855,

$$\alpha = 20^{h}\ 8^{m}\ 30^{s}\cdot200,$$

$$\delta = 88^{\circ}\ 52'\ 32''\cdot30,$$

and the following values of the precession-constants $z+\lambda$, $z'-\lambda'$, θ, derived from the data of Bessel's *Tabulæ Regiomontanæ*:—

	$z+\lambda$	$z'-\lambda'$	θ
	$'$ $''$	$'$ $''$	$'$ $''$
1755	−38 25·02	−38 19·54	−33 25·869
1790	25 0·01	24 53·31	21 43·786
1800	21 10·03	21 2·84	18 23·123
1810	17 20·07	17 12·35	15 2·536
1815	15 25·09	15 17·09	13 22·245
1835	7 45·21	−7 35·94	−6 41·104
1855	−5·37	+5·37	0·000
1875	+7 34·42	7 46·85	+6 41·065

By the help of these formulæ,

$$A = \alpha + z + \lambda,$$

$$m \cos M = \cos\delta \cos A,$$

$$m \sin M = \sin\delta,$$

$$\cos\delta' \sin A' = \cos\delta \sin A,$$

$$\cos\delta' \cos A' = m \cos(M+\theta),$$

$$\sin\delta' = m \sin(M+\theta),$$

$$\alpha' = A' + z' - \lambda',$$

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were obtained the following values of α', δ', the right ascensions and declinations for the various equinoxes and equators, corresponding to the fixed values for 1855 :—

	α'			δ'		
	°	′	″	°	′	″
1755	319	24	5·34	88	30	25·24
1790	314	28	52·1	88	38	58·86
1800	312	52	28·4	88	41	17·41
1810	311	10	7·2	88	43	31·70
1815	310	16	36·49	88	44	37·13
1835	306	26	4·54	88	48	46·21
1855	302	7	33·00	88	52	32·30
1875	297	19	34·67	88	55	51·35

	A.R. in Time.		
	h	m	s
1755	21	17	36·356
1790	20	57	55·47
1800	20	51	29·89
1810	20	44	40·48
1815	20	41	6·433
1835	20	25	44·303
1855	20	8	30·200
1875	19	49	18·311

The values of the precession, secular variation, and Argelander (Tiele's) third term were then computed for the last four dates, as follows:—

	In A.R.		
	Precession.	Secular Variation.	Third Term.
	s	s	s
1815	−43·4488	−25·772	−209·2
1835	−48·8398	−28·055	−165·1
1855	−54·6192	−29·562	− 77·0
1875	−60·5727	−29·678	+ 67·5

In computing these quantities, the tables by Menten and Tiele in vol. vii. of the *Bonn Observations*, and those there referred to in the *Washington Observations* for 1847, were employed; the quantities being modified where necessary to correspond to Bessel's m and n from the *Tabulæ Regiomontanæ*.

In calculating the secular variations, I found it necessary to compute the coefficient of $\tan^2 \delta$ to more decimals than the table gives. This coefficient is

For 1755	[8·11432] sin 2α
1875	[8·11381] sin 2α.

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Menten's table gives to four places the logarithm of [8·1139] sin 2α.

The similar coefficients in declination are

	Precession.	Secular Variation.	Third Term.
	"	"	"
1815	+12·9671	−4·838	−69·2
1835	11·9115	−5·734	−79·8
1855	10·6604	−6·751	−88·7
1875	+ 9·2051	−7·752	−93·8

The third term, as calculated by the help of Tiele's table, is not to be depended upon in its last place at so high northern declinations; but as it is the coefficient of $\left(\frac{t}{100}\right)^3$ in the development according to powers of the time, this does not matter much for an interval of ten years before or after the epoch.

Multiplied by ·06 this "third term" gives us the secular variation of the secular variation, as can be easily shown by Taylor's theorem, and roughly controlled by the values given.

Of more consequence is sometimes the fourth term in the development. If we wish to proceed, say from 1835 to 1845, we can interpolate the third term roughly for one-fourth the interval; or in this case from 1835 to 1837·5; which gives us −156ˢ for its value at the last date. Using this value we shall take into account the fourth term so far as is needful. Then the A.R. for 1845 will be

	h	m	s
A.R. 1835	20	25	44·303
10 × Precession		−8	8·398
$\frac{1}{2}$ × Secular Variation			−14·027
$\left(\frac{10}{100}\right)^3$ × Third Term			−0·156
A.R. 1845	20	17	21·72

The next step is to compute the effect of proper motion, assumed as −0ˢ·073 and +0″·01 annually, referred to the planes of 1855.

To refer them to any other plane the formulæ will be

$$\sin \xi = \frac{\sin \theta \sin A'}{\cos \delta},$$

where A′ is $\alpha' - z' + \lambda'$ before used, and δ is the declination for 1855.

Then if

$$\Delta\alpha = -0^s\cdot073\,(t-1855) = -1''\cdot095\,(t-1855),$$
$$\Delta\delta = +0''\cdot01\,(t-1855),$$

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we shall have

$$\Delta\alpha' \cos\delta' = \Delta\alpha \cos\delta \cos\xi + \Delta\delta \sin\xi,$$
$$\Delta\delta' = -\Delta\alpha \cos\delta \sin\xi + \Delta\delta \cos\xi.$$

These formulæ give us the following results:—

	$\Delta\alpha'$	$\Delta\delta'$	ξ
	s	s	° ′
1790	+51·41	−0·952	+13 11·44
1800	+45·82	−0·774	+11 27·32
1810	+39·47	−0·605	+ 9 37·27
1815	+36·00	−0·525	+ 8 39·90
1825	+28·41	−0·373	+ 6 40·29
1835	+19·91	−0·233	+ 4 33·94
1845	+10·45	−0·109	+ 2 20·59
1855	0·00	0·000	
1865	−11·46	+0·091	− 2 27·97
1875	−23·89	+0·161	− 5 3·39
1885	−37·25	+0·210	− 7 46·24

For 1755 the values of $\Delta\alpha'$ and $\Delta\delta'$, $+65''\cdot96$ and $-1''\cdot63$ respectively, were obtained by a direct computation, using

$$\alpha = 302\ 9\ 22\cdot50$$

and

$$\delta = 88\ 52\ 31\cdot30.$$

The next step is the comparison of these computed places with observation, carried out as follows:—

	Computed A.R.	Computed Decl.	
	° ′ ″	° ′ ″	
1790	314 28 52·1	+88 38 58·86	With precession alone.
	+51·41	−0·95	Effect of proper motion.
	314 29 43·51	+88 38 57·91	Sum: Computed place.
	h m s		
	20 57 58·90		
Fedorenko's Catalogue	20 58 2·38	88 38 57·76	Observed place.
Computed—Observed	−3·48	+0·15	

In this case the proper motion from the date of observation to the epoch is neglected.

In the following table are given the comparisons for which I have materials at hand. The principal observations wanting are Piazzi, Greenwich, 1868–1872, and various modern series not yet combined into Catalogues; a few positions here given are derived at second-hand from former investigations by myself or others; and several of the places in the table need revision and correction for the adopted nutation:—

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	$\Delta\alpha$ s	$\Delta\alpha \cos\delta$ "	$\Delta\delta$ "	No. Obs.
Bradley 1755	−0·79	−0·31	+0·21	3
Fedorenko 1790*	−3·48	−1·23	+0·15	2
Groombridge 1807*	+1·75	+0·58	+0·8	Error in Radcliffe?
Struve 1815*	+0·4	+0·13	...	4
Schwerd 1828*	0·0	0·00	−0·10	11
Catalogus Aboensis 1830	+0·04	+0·01	+0·25	17
Cambridge 1832	+3·67	+1·15	...	14
Pond 1833	−0·77	−0·24	+0·25	20, 24
Cambridge 1835	...	...	+0·20	17
Taylor 1835	+0·97	+0·30	+1·13	2, 3
Henderson 1838	−0·35	−0·11	−0·08	26, 39
Challis 1838	...	...	+0·30	32
Airy 1840	−1·54	−0·47	+0·05	25, 77
Armagh 1843	+0·56	+0·17	...	36
Radcliffe 1843	...	...	+0·48	158
Airy 1845	−1·55	−0·47	+0·21	15, 56
Pulkova 1845	+0·17	+0·05	+0·16	21, 5
Radcliffe 1847	−0·26	−0·08	...	111
Airy 1851	−1·27	−0·38	−0·08	49, 106
Armagh 1851	...	...	−0·26	5
Carrington 1855	+0·3	+0·09	−0·3	27
Yarnall 1856	...	...	+0·18	117
Airy 1857	−0·72	−0·21	+0·19	80, 123
Paris 1857*	+0·23	+0·07	−0·24	50, 59
Airy 1864	+0·40	+0·12	+0·24	30, 84
Pulkova 1868	+0·24	+0·07		
Leiden 1868	...	...	+0·14	40
Washington 1870	−0·14	−0·04	+0·19	104, 177
Rogers 1872	−0·84	−0·24	+0·18	20, 12
Airy 1874	+0·95	+0·27	−0·16	36, 91

The authorities marked with an asterisk are taken at second-hand from others' investigations or my own. There seems to be an error of 1″ to 2″ in the reduction of Groombridge's Polar Distance to 1845, as found in the Radcliffe Catalogue.

To reduce the values of $\Delta\alpha$ to the parallel the following little table of log (15 cos δ) was found serviceable:—

Year.	(log 15 cos δ.)	Year.	(log 15 cos δ.)
1755	9·592	1835	9·492
1790	9·548	1855	9·469
18	9·516	1875	9·447

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My first intention was to assign weights and proceed to a solution of equations of condition, thus correcting the assumed position; but the resulting corrections were found to be very small, and I have judged it best to wait until I can add all the observations to be found in Catalogues which I do not possess. The experiments which I made led mainly to the conclusion that the formulæ heretofore given cannot be much improved at present, at least with any certainty.

In carrying the calculations forward for the next few years, the following values for 1885, depending on the same elements, will be found useful:—

	A.R. h	m	s	Decl. °	′	″
Value without P.M. from 1855	19	38	57·84	+88	57	19·39
Annual Precession			−63·5110			+8·3917
Secular Variation			−28·998			−8·415
Third Term			+162·2			−93·6

The effect of P.M. from 1855 up to the year of observation should be interpolated from the previous table and added to the values derived from these numbers.

In case the arrangement of the calculation given in this paper shall save anyone else the annoyances which I have undergone in the solution of similar problems, my object will have been gained; the points of originality in the process are of course few and small.

The star's place itself would seem not to require so many observations as have been made for a few years, but rather such as shall individually possess the greatest possible accuracy.

Williams College,
Williamstown, Mass., U.S.A.,
May 15, 1878.

A Reply to Mr. Proctor's Note in the March Number of the Monthly Notices. By E. J. Stone, M.A., F.R.S., Her Majesty's Astronomer, Cape of Good Hope.

1. Mr. Proctor's remarks in the March No. on my paper in the January No. appear to call for some reply on my part.

In Mr. Proctor's paper in the Supplementary No. of the *Notices*, 1877, he says that he has proved "that by no optical contrivances can apparent brightness be increased, though quantity of light can, of course, be greatly increased." I stated in my note in the January No. that this statement requires qualification; for, if it were true, why could not the satellites of *Mars* be as easily seen with a 4-inch telescope as with the 26-inch of the Washington Observatory? Mr. Proctor's answer to this is that my question contains its own answer, viz. "that quantity of light is, of course, greatly increased when the larger telescope is used." But the satellites would not be visible with the large telescope rather than with the small one unless their apparent

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brightness was also increased. It will be seen, therefore, that Mr. Proctor's answer directly proves my statement that his general proposition requires qualification and is not applicable to such objects as the satellites of *Mars*; and anyone who will refer to my paper in the January No. will see that my object in bringing this point forward was simply to show that Mr. Proctor's statement required qualification before we could apply it to the case of distant stellar clusters, which was the subject of consideration in my paper. Although I have not mentioned anything about the theory of diffraction disks directly, anyone acquainted with the subject would see at once that it was the basis upon which my views rested, and the reason of my referring to telescopes of a small aperture and low magnifying power in relation to some "Statements of Mr. Proctor on Irresolvable Clusters."

2. My original paper on "A cause for the appearance of bright lines in the Spectra of Irresolvable Star Clusters" has been misunderstood, and therefore could not have been explained in sufficient detail; but on reference to my original paper in the *Proceedings of the Royal Society*, or the extract from it given in the January No. of the *Notices*, it will be seen that I state that "each star, if isolated, would be surrounded by its own gaseous envelope. These gaseous envelopes might, in the case of a cluster, form over the whole or a part of the cluster a *continuous mass* of gas"; and it is expressly stated that it is with reference to such clusters that the light of the gaseous envelope might ultimately predominate over that of the stellar masses. If the phrase "a continuous mass of gas" means anything, it means what I assert I intended it to mean in the January No., viz. that the clusters were physically close and not merely optically close, and I think that the qualifications with which Mr. Proctor admits that he does not doubt that I really meant in my original paper what I asserted that I meant in the January No. would have been better omitted.

1878, *May* 13.

Note on Dr. W. Erck's Observations of the Outer Satellite of Mars. By A. Marth, Esq.

In consequence of an unnoticed innovation in the dating of two of Dr. Erck's observations of the satellite of *Mars*, the computed angles of position given on page 365, for September 16 and 26, do not refer to the times of the observations, which were really made 24 sidereal hours earlier. When making the computations, I had not the text of Dr. Erck's communication before me, but merely an extract containing the assigned times and position angles, and therefore I took September 16, $1^h\ 0^m$ G.S.T. to mean, as usually, $1^h\ 0^m$ G.S.T. on September 16, as I was not aware of Dr. Erck's deviation from the customary reckoning of dates. It appears, however, from a reference to the text, and from a further communication made

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by Dr. Erck, that in order to conform with established custom the dates should be September 15 and 25, and that, in consequence, for the lines

	h	m		Obs. °	Comp. °
Sept. 16	1	0	G.S.T.	238	267·1
26	0	35		255	322·4

must be substituted

	h	m		°	°
Sept. 15	1	0		238	240·2
25	0	35		255	256·1

so that, instead of the previous discordance, there is excellent agreement between the observed and computed angles of position.

Mr. W. S. Franks, of Leicester, has presented to the Society, through the Rev. T. W. Webb, a "Catalogue of the Colours and Magnitudes of 3,890 Stars between the North Pole and 25° South Declination," from observations made by him between July 24, 1877, and May 3, 1878, with an equatoreally mounted refractor of 5 inches aperture and 78 inches focal length, the object-glass being a fine one, by Wray. The eye-piece generally employed was an Aplanatic, by Horne and Thornthwaite, giving a magnifying power of 105, higher powers being occasionally used in the case of close double stars.

In regard to the work Mr. Franks says:—"My method of observing was to take one hour in R.A. as the limit of the sweeps, and commencing at 25° South Declination, examine each zone of 5° in declination at a time, gradually working up to the Pole. In doing so I endeavoured, as much as possible, to follow the natural grouping of the stars in preference to a rigid adherence to their strict sequence in Right Ascension." The independent estimates of magnitude were checked by a comparison with surrounding stars, for which Mr. Franks found a good opera-glass very useful. The works most frequently used for reference were Proctor's large and small Star Atlases, the large maps of the Useful Knowledge Society, Argelander's *Uranometria Nova*, the British Association Catalogue, and Webb's *Celestial Objects for Common Telescopes*."

The Catalogue is arranged in accordance with the method adopted in making the observations, a method perhaps not the most convenient for ready reference, but, as the stars are arranged in hours of R.A. and the name and B.A.C. number of each is given, the identification of any object is sufficiently easy. Each page contains seven columns, giving the B.A.C. number, the constellation, the B.A.C. magnitude, the observed magnitude, the colour, remarks, and the date of observation.

The work, which is the outcome of a good deal of painstaking labour on the part of Mr. Franks, will be bound and placed in the Library for convenience of reference by workers in the same field of observation.

G. K.

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THE SPHEROMETER CALIPER.

Fig. 1.

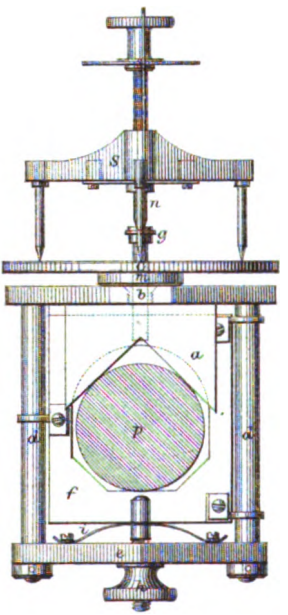


Fig. 2.

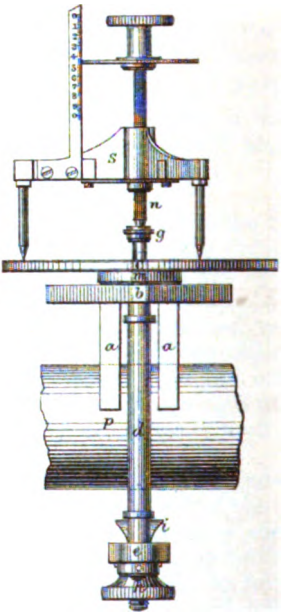


Fig. 3.

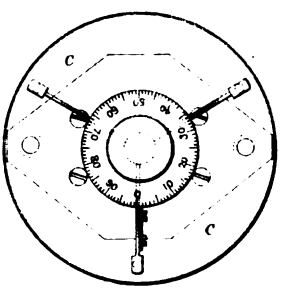
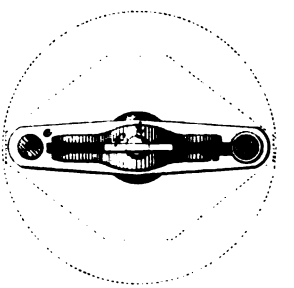


Fig. 4.



Scale

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MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

VOL. XXXVIII. SUPPLEMENTARY NOTICE. No. 9.

On the Measurement of the Inequality of Pivots of Transit Instruments by means of the Spherometer.

By Professor Wm. Harkness, U.S. Navy.

In the autumn of 1875 it became necessary to determine the inequality of pivots of the eight Transit Instruments which had been used by the United States transit of *Venus* parties; and to have done this in the ordinary way, by means of the spirit level, would have occupied not less than three weeks. This was more time than I could conveniently spare for the purpose, and after some thought I contrived an instrument with which the required determinations were most accurately made, and the time expended was only an hour for each transit. This instrument was first tried on February 28, 1876, and as some copies of it have already been made for other astronomers, and it seems to be coming rapidly into use in the United States, perhaps a detailed description of it may be acceptable to the Royal Astronomical Society.

The spherometer-caliper, for such is the name of the instrument, is shown in the accompanying plate: fig. 1 being a front elevation; fig. 2 a side elevation; fig. 3 a plan; and fig. 4 a section through the point *f*, fig. 1. The lettering is the same on all the figures. The foundation plate, *b*, of the instrument carries two cylindrical guides, *d*, *d*, which are connected at their lower ends by the bar *e*. Into the foundation plate is screwed the brass piece *m*, to which is cemented the thick circular glass plate *c*. The two Ys, *a*, *a*, are also firmly screwed to the foundation plate. The brass plate *f* slides freely up and down between

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the guides *d*, *d*; being kept in their plane by three loops, two of which pass around the right-hand guide, and one around the left-hand guide, as shown in the figure. The brass rod *g*, which passes through the piece *m* and the plate *c* without touching either of them, is firmly attached to the upper end of the plate *f*, and moves with it; while to the lower end of *f* is attached a second short brass rod which passes freely through the bar *e* and carries the nut *h*. There is an aperture in the plate *f* sufficiently large to permit the free passage of such transit pivots as the caliper is intended to measure.

In using the instrument the plate *f* is depressed by means of the nut *h* until *p*, one of the pivots whose inequality is to be measured, passes freely under the Ys, *a*, *a*. Then, the Ys having been properly adjusted upon the pivot, *h* is loosened, and the flat edge of the aperture in *f* is forced against the under side of the pivot by the spring *i*. Finally, the elevation of the end of the rod *g* above the glass plate *c* is accurately measured by means of a small spherometer. This process having been repeated upon the other pivot, the inequality of the two pivots will evidently be a function of the difference of the two readings of the spherometer. To insure the greatest possible accuracy of measurement, the glass plate *c* must be entirely free from strain, and the flat surface of the opening in *f* must be pressed with the same degree of force against the under surfaces of the two pivots. Both these conditions are evidently fulfilled in the construction of the instrument; and, as an additional precaution, the top of the rod *g* is capped with a carefully ground agate, upon which the point of the spherometer screw rests.

To find the relation between the difference of the two readings of the spherometer and the inequality of the pivots upon which they were made, put

$2i$ = the angle of the Ys of the caliper:
n = the difference between the readings of the spherometer on the two pivots:
R = the linear value of one revolution of the spherometer screw:
l = the distance between the pivots of the transit instrument:
p = the inequality of pivots; expressed in seconds of time, and to be considered positive when the eastern pivot is the largest.

Then, if the radius of the pivot undergoing measurement is r, the vertical distance from the angle of the Ys to the centre of the pivot will be $\frac{r}{\sin i}$; and the vertical distance from the centre of the pivot to the flat surface of the aperture in *f* will be r. Hence, if the vertical distance from the upper surface of the glass plate *c* to the angle of the Ys is C, the vertical distance from the upper surface of *c* to the flat surface of the aperture in *f* will be

$$C + r + \frac{r}{\sin i} = C + r\left(\frac{1+\sin i}{\sin i}\right). \qquad (1)$$

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Similarly, for the other pivot, whose radius is r', the distance will be

$$C + r'\left(\frac{1+\sin i}{\sin i}\right). \qquad (2)$$

The difference between (1) and (2) is

$$(r-r')\left\{\frac{1+\sin i}{\sin i}\right\} \qquad (3)$$

which is the space moved through by the plate f when the second pivot was substituted for the first. As the plate f carried the rod g with it, this space must be equal to the linear value of the difference of the readings of the spherometer upon the two pivots. Hence

$$(r-r')\left\{\frac{1+\sin i}{\sin i}\right\} = nR, \qquad (4)$$

and therefore

$$r-r' = \frac{nR\sin i}{1+\sin i}, \qquad (5)$$

which is the inequality of pivots, expressed linearly. To convert it into seconds of time it must be divided by $l.15\tan 1''$, and thus we obtain finally

$$p = n\frac{R\sin i}{l(1+\sin i)\,15\tan 1''}. \qquad (6)$$

By the kind permission of the Superintendent of the U.S. Naval Observatory, I am enabled to give some examples of the actual working of the spherometer-caliper. The first is the measurement, made March 20, 1876, of the inequality of pivots of the portable transit instrument, Stackpole & Bro., No. 1502.

Position of Pivot. °	Clamp Pivot. r	Lamp Pivot. r	n. r	p. s
180	43·551	43·539		
	·550	·540		
	·549	·540		
	43·5500	43·5397	0·0103	0·031
225	43·549	43·536		
	·551	·540		
	·551	·540		
	43·5503	43·5387	0·0113	0·034

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Position of Pivot. °	Clamp Pivot. r	Lamp Pivot. r	n. r	p. s
270	43·551	43·540		
	·551	·539		
	·550	·540		
	43·5507	43·5397	0·0110	0·033
315	43·548	43·540		
	·552	·540		
	·553	·541		
	43·5510	43·5403	0·0107	0·032
			Mean value of p =	0·032

Four diameters were measured in each pivot, their positions being indicated by the readings of the finding circle, which are contained in the first column of the table above. The readings of the spherometer upon the clamp pivot are contained in the second column, those upon the lamp pivot in the third column; and the resulting values of n and p are given in the fourth and fifth columns. The mode of measuring each diameter was as follows: The transit instrument being upon its reversing carriage, so that the pivots were entirely free, the axis was turned until that diameter of the pivot which it was desired to measure was vertical. Then the caliper was slipped on to the pivot and adjusted so that the surface of the plate c was perfectly level, after which the spherometer was placed upon it and very carefully read. This process was repeated three times; the caliper being removed from the pivot after each reading, so as to render the readings as independent of each other as possible. The value of n for any given diameter was, of course, the difference between the mean of the three readings upon the clamp pivot and the mean of the three corresponding readings upon the lamp pivot. To find p we require certain constants, the values of which in the present instance are

$$2i = 90°, \quad R = 0{\cdot}02697 \text{ cm.}, \quad l = 50{\cdot}65 \text{ cm.}$$

Substituting these values in equation (6), we obtain

$$p = {}^{s}_{r}\,3{\cdot}033n,$$

and accordingly, the values of p in the table above have been obtained from those of n by multiplying the latter quantities by 3·033. A comparison of the numbers in the second and third columns shows that the clamp pivot is the largest.

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Our second example of the working of the spherometer-caliper is the measurement, made February 18, 1878, of the inequality of pivots of the portable transit instrument, Stackpole & Bro., No. 1506.

Position of Pivot. °	Clamp Pivot. r	Lamp Pivot. r	a. r	p. s
180	43·237	43·218		
	·232	·219		
	·235	·217		
	43·2347	43·2180	0·0167	0·051
225	43·235	43·222		
	·238	·221		
	·238	·224		
	43·2370	43·2223	0·0147	0·045
270	43·237	43·215		
	·235	·219		
	·238	·218		
	43·2367	43·2173	0·0194	0·059
315	43·236	43·216		
	·239	·216		
	·238	·211		
	43·2377	43·2143	0·0234	0·071
			Mean value of p =	0·056

Upon reducing these measurements, my first impression was that the large value of p at 315° must have been erroneous, and I remeasured the pivot inequality at that point, with the result, $p = 0''{\cdot}075$. This confirmed the first measurement, and showed beyond all possibility of doubt that one, or perhaps both of the pivots were elliptical. An examination of the spherometer readings upon the clamp pivot proves that it is round, at least within the limits of error of observation; but with the lamp pivot the case is different. The largest spherometer reading upon it is at 225°, and the smallest at 315°. These are therefore the approximate positions of the major and minor axes of the ellipse. The difference of the spherometer readings at these points is $0^{r}{\cdot}0080$. Substituting this in equation (5), we find

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that the semi-major axis exceeds the semi-minor one by 0·000089 cm.—a small quantity truly, but one about which there can be no mistake. This degree of ellipticity is so slight that it may be neglected in a portable instrument, and accordingly we adopt the mean value of p, which is 0″·056; the clamp pivot being the largest.

More examples might be given, but the above suffice to show that, for measuring pivot inequalities, the spherometer-caliper possesses the following advantages over the spirit level:—1st, It is more accurate; 2nd, it shows the exact form of each pivot; 3rd, it is at least ten times more expeditious.

To secure the advantage of accuracy to its fullest extent the following points must be attended to:—1st, The spherometer must be as light as possible: the one shown in the plate weighs only 37·10 grammes; 2nd, the screw of the spherometer must be entirely free from dead motion; 3rd, the surface of the glass plate c must always be truly levelled before applying the spherometer to it. In a properly constructed instrument there should be no difficulty about the second of the above conditions, but unfortunately few spherometers are properly constructed. The makers generally provide some means of tightening the main nut, thinking in that way to destroy the dead motion; but if any serious attempt is made to use this arrangement, it will be found that long before the dead motion disappears, the motion of the screw becomes so stiff as to render the instrument totally unmanageable. In my own instrument, immediately below the main nut there is a supplementary one with three elastic arms, which are attached to the frame of the instrument in such a manner that any required strain can be put upon them. The spherometer screw passes through both these nuts, and by carefully adjusting the tension on the arms of the supplementary one, which is two or three threads below the other, the two can be so pressed toward each other as to eliminate all dead motion, and yet leave the movement of the screw as light as may be desired. The importance of the third condition mentioned above depends upon the fact that all well-made spherometers are so balanced that when they stand upon a level surface their centre of gravity is in the screw. Consequently, when the screw comes in contact with the object to be measured, the three feet of the instrument will lift simultaneously if it is standing upon a level surface, but if it is standing upon a surface ever so little inclined, there will be a tendency for one foot to lift before the others, and the accuracy of measurement will be greatly impaired.

The spherometer-caliper shown in the drawing which accompanies this paper is the first one ever constructed, and as it is not furnished with any means of varying the size of the aperture in the plate f, it is only applicable to pivots whose diameter is not far from 3·40 cm. In a much larger instrument, recently made for Professor C. A. Young, of Princeton College, the flat edge of the aperture in the plate f consists of a bar which can be

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adjusted to any desired height upon the plate, and thus the instrument is rendered capable of measuring pivots of almost any size.

Washington, 1878, May 22.

Spectroscopic Results for the Motions of Stars in the Line of Sight, made at the Royal Observatory, Greenwich. III.

(*Communicated by the Astronomer Royal.*)

The Results for Star Motions here given are in continuation of those printed in the *Monthly Notices* for 1876, May and November, and the general arrangement is the same.

Up to 1877, May 2, the observations were made with the ten-prism spectroscope (two or four prisms only being used for star work), and the method of observation has been sufficiently described in preceding communications. On 1877, August 23, the half-prism spectroscope was brought into use, but the observations suffered interruption during the oppositions of *Mars* and *Saturn*, and it was not till 1877, November 14, that this work was again brought into regular train. The principle of the new spectroscope is fully described in a paper in the *Proceedings of the Royal Society*, No. 179, 1877; and it may be sufficient to state here that the full train consists of two compound half-prisms, each being the half of a direct-vision prism, cut in two by a plane perpendicular to the base. Either one or two of the half-prisms can be used at pleasure: with one half-prism a dispersion of 20° from A to H, equivalent to four prisms of 60°, is obtained; and with two half-prisms a dispersion of 75° from A to H, equivalent to fifteen prisms of 60°. The number of half-prisms used is denoted by the Roman numerals I and II. The full dispersion of 75°, which is far greater than any hitherto used for stellar observations, has been applied recently to some of the brighter stars, including one of the second magnitude, and no difficulty has been experienced in making satisfactory comparisons with this high power. As a check against possible instrumental error arising from maladjustment of the spectroscope, comparisons have been regularly made, either before or after the observations, with the spectrum of the Moon or with that of the sky, the results of which are given at the end of the table.

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Motions of Stars in the Line of Sight in Miles per Second.

(+ denotes Recession; − Approach.)

One division in the width of the slit of the Ten-prism Spectroscope corresponds to 0·0013 inch; one revolution of the slit screw of the Half-prism Spectroscope corresponds to 0·01 inch; the weights are on a scale 1–5 for *each* observation; the Means have been taken for the whole series of observations, including former results.

The initials W.C. and M. are those of Mr. Christie and Mr. Maunder respectively.

α Andromedæ.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	d d					
1876 Nov. 17	M.	2	2	2 & 4	6	2·9 & 3·1	F	+11·6	—	−69·1	Direct comparison.
Dec. 6	M.	2	3	4	6	2·8 & 4·0	F	+15·1	−39·1	−37·5	Indirect comparison; star line very difficult to bisect.
				Mean, Ten-prism Spectroscope					−34·4	−55·2	Weight 16.

α Arietis.

1878 Feb. 6	M.	3	6	I	5	...	b_1	+18·2	+63·1	+45·5	Indirect comparison; spectrum rather tremulous.

Aldebaran.

1877 Dec. 31	W.C.	2	4	I	95	...	b_1	+10·1	+22·5	+ 1·8	Indirect comparison.
31	M.	2	4	I	95	—	b_1	+10·1	+21·2	+18·5	Indirect comparison.

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1878 Jan. 10	M.	2	4	I	95	...	b_1	+12·8	+20·8	+20·1	Indirect comparison; spectrum very tremulous; definition poor.
23	M.	2	2	I	95	...	b_1	+15·6	+38·2	+41·3	Indirect comparison; definition very bad.
29	M.	2	2	I	95	...	b_1	+16·6	+15·9	+ 4·8	Indirect comparison; spectrum very tremulous; observation very difficult.
Apr. 8	M.	2	6	II	5	$0^r\cdot072$	b_1	+13·8	+15·2	+18·0	Indirect comp.; definition only moderate.
					Mean	Ten-prism Spectroscope			+35·1	+48·7	Weight ⅛.
						Half-prism Spectroscope			+20·8	+16·4	Weight 22.
								Capella.			
1876 Nov. 1	M.	1	2	2	6	$3\cdot8^d$	F	−11·1	...	+67·0	Direct comparison.
17	M.	2	3	4	6	3·8 & 1·4	F	− 7·0	...	+31·4	Direct comparison.
17	M.	1	1	6	6	2·1	b_1	− 7·0	...	+61·2	Direct comparison.
1878 Mar. 15	M.	2	8	I	5	$0^r\cdot085$	b_1	+17·0	+39·9	+42·0	Indirect comparison.
16	W.C.	3	2½	I	120	0·080	F	+17·0	...	+34·0	Direct comparison.
16	W.C.	3	7	I	120	0·073	b_1	+17·0	...	− 2·6	Indirect comp.; the bright line of the micrometer being ragged, the comparison was uncertain.
25	M.	2	10	I	95	0·151	F	+16·4	+15·9	+20·9	Indirect comp.; definition very good.
Apr. 1	M.	2	6	I	5	0·128	b_1	+15·7	+55·8	+57·0	Indirect comp.; spectrum very tremulous.
					Mean	Ten-prism Spectroscope			+16·0	+30·9	Weight 18.
						Half-prism Spectroscope			+33·9	+27·3	Weight 29.

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β Orionis.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	d					
1877 Mar. 10	M.	2	3	4	2½	2·5	F	+15·8	+ 7·2	+ 9·4	Indirect comparison.
Dec. 31	M.	3	6	I	95	...	F	+ 7·0	+20·0	+14·5	Indirect comparison; definition fair.
1878 Jan. 10	M.	3	7	I	95	...	F	+ 9·5	+26·0	+21·6	Indirect comparison; definition poor.
23	W.C.	3	9	I	95	...	F	+12·2	...	+17·9	Indirect comparison for first two obs.; direct for last.
29	M.	3	3	I	95	...	F	+13·2	+14·1	+13·0	Indirect comparison; spectrum very tremulous; definition very poor.
Feb. 6	M.	2	2	I	5	...	F	+14·4	+34·9	+38·4	Indirect comp.; spectrum very tremulous.
				Mean	Ten-prism Spectroscope.				+18·5	+20·2	Weight 8.
					Half-prism Spectroscope				+23·0	+19·1	Weight 22½.

β Tauri.

1878 Jan. 29	M.	3	3	I	95	...	F	+14·2	− 9·8	−10·2	Indirect comparison; spectrum steady; definition fair.

γ Orionis.

1878 Jan. 29	M.	5	10	I	95	...	F	+14·0	− 0·5	− 2·4	Indirect comparison.
29	M.	1	2	I	95	...	F	+14·0	....	−14·0	Indirect comparison.
Feb. 17	W.C.	2	4	I	0	...	F	+16·9	+ 2·8	+11·0	Indirect comparison.
				Mean: Half-prism Spectroscope					+ 0·4	− 0·5	Weight 15.

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δ Orionis.

1877 Dec. 31	M.	1 est.	1	I	95	...	F	+ 5·9	...	−18·0	Direct comparison.
1878 Jan. 10	M.	2	1	I	95	...	F	+ 8·7	+21·1	+17·5	Indirect comparison; star faint; definition bad.
23	W.C.	2	3	I	95	...	F	+11·9	...	+ 0·0	Indirect comparison.
Feb. 18	M.	2	2	I	6	...	F	+16·1	+34·6	+45·3	Indirect comparison.
					Mean	Ten-prism Spectroscope			+25·4	+21·9	Weight 7.
						Half-prism Spectroscope			+30·1	+12·8	Weight 5.

ε Orionis.

1877 Mar. 10	M.	2	0	4	2½	...	F	+16·9	(−46·1)	...	Star line scarcely visible; observations discordant.
10	M.	2	6	2	2½	4·6 d	F	+16·9	−48·7	−61·4	Indirect comparison.
Dec. 31	M.	2	3	I	95	...	F	+ 5·5	+29·9	+18·7	Indirect comparison; star line very faint.
1878 Jan. 23	W.C.	1	2	I	95	...	F	+11·6	...	+24·5	Indirect comparison.
Feb. 18	M.	3	3	I	6	...	F	+15·9	+20·9	+23·3	Indirect comparison.
					Mean	Ten-prism Spectroscope			−48·7	−61·4	Weight 6.
						Half-prism Spectroscope			+25·4	+21·9	Weight 7.

ζ Orionis.

1878 Feb. 18	M.	2	2	I	6	....	F	+15·7	+17·4	+29·8	Indirect comparison.

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α *Orionis.*

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	d d					
1877 Feb. 28	M.	3	3	2	6	1·4 & 2·0	b_4	+17·2	+19·2	+17·1	Indirect comparison; definition poor; Moon bright.
						d					
Mar. 10	M.	2	8	4	2½	2·6	b_4	+17·8	+22·1	+11·0	Indirect comparison; star line seen well.
Dec. 31	M.	2	4	I	95	...	b_4	+ 4·2	+16·9	+14·9	Indirect comparison; spectrum tremulous; definition variable.
1878 Feb. 18	M.	3	12	I	6	...	b_1	+16·1	+33·9	+36·4	Indirect comparison; definition good.
						r					
Mar. 15	M.	2	3	I	5	0·098	b_4	+17·9	+16·8	+15·8	Indirect comp.; spectrum tremulous.
Apr. 1	M.	3	12	I	5	0·128	b_1	+17·1	+20·5	+19·3	Indirect comparison; spectrum tremulous, but definition fair.
8	M.	3	8	II	5	0·072	b_1	+16·3	+ 9·0	+ 9·5	Indirect comp.; spectrum tremulous.
					Mean	Ten-prism Spectroscope			+21·3	+12·7	Weight 11.
						Half-prism Spectroscope			+21·6	+21·8	Weight 39.

β *Aurigæ.*

						r					
1878 Apr. 11	M.	4	2	I	5	0·072	F	+15·6	+11·5	+22·3	Indirect comp.; star line very difficult.

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Sirius.

1877	Feb. 28	M.	2	5	4	6	$5{\cdot}1^{d}$ & $2{\cdot}7^{d}$	F	+12·2	+ 9·3	+14·5	Direct comparison; definition poor; Moon bright.
1878	Feb. 17	W.C.	4	5	I	0	...	F	+10·5	+22·1	+ 8·3	First two comps. direct; last two indirect.
	18	M.	2	6	I	6	...	F	+10·7	+30·4	+32·8	Indirect comparison.
	Mar. 25	M.	2	4	I	95	$0{\cdot}151^{r}$	F	+14·1	+23·2	+41·8	Indirect comparison; spectrum very tremulous.
							Mean { Ten-prism Spectroscope			+19·9	+24·8	Weight 29.
							Mean { Half-prism Spectroscope			+25·7	+27·0	Weight 15.

Castor.

1877	Mar. 10	M.	1	2	4	$2\frac{1}{2}$	$3{\cdot}4^{d}$	F	+16·2	− 3·9	+ 5·7	Indirect comparison.
1878	Feb. 25	M.	2	2	I	95	...	F	+13·9	+26·9	+38·4	Indirect comparison; star line ill defined.
	Mar. 25	M.	2	7	I	95	$0{\cdot}141^{r}$	F	+17·6	+33·7	+30·5	Indirect comparison.
	30	M.	2	2	I	5	0·073	F	+17·9	+54·4	+43·7	Indirect comparison; definition poor; and observation difficult.
							Mean { Ten-prism Spectroscope			+25·3	+22·3	Weight 19.
							Mean { Half-prism Spectroscope			+36 2	+34·3	Weight 11.

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Procyon.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	d					
1877 Mar. 5	M.	2	5	2	10	2·0	F	+14·0	+30·7	+19·6	Indirect comparison; spectrum bright and steady.
1878 Feb. 25	M.	2	6	I	95	...	F	+12·3	+15·6	+20·0	Indirect comp.; spectrum tremulous.
Mar. 16	W.C.	2	0	I	120	r 0·073	b_1	+15·7	(−59·8)	(−25·2)	Indirect comp.; star lines very faint; observations discordant *inter se*.
25	M.	2	8	I	95	0·141	F	+16·7	+ 7·5	+10·3	Indirect comparison; definition good.
30	M.	2	5	I	5	0·123	F	+17·2	+39·6	+46·2	Indirect comp.; definition poor.
Apr. 20	M.	2	4	II	5	0·100	F	+17·4	+16·4	+34·2	Indirect comparison; much interrupted by light cloud.
				Mean	Ten-prism Spectroscope				+42·8	+35·3	Weight 24.
					Half-prism Spectroscope				+22·0	+22·6	Weight 23.

Pollux.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
1877 Feb. 28	M.	2	2	2	6	d d 1·0 & 1·8	b_1	+13·8	−52·1	−89·8	Indirect comparison; definition poor; Moon bright.
Mar. 10	M.	2	6	4	2½	d 2·6	b_1	+15·7	−30·2	−45·7	Indirect comparison.
1878 Feb. 18	M.	4	12	I	6	...	b_1	+11·5	+ 6·2	+ 7·7	Indirect comparison; definition variable; observations discordant *inter se*.
25	M.	1	1	I	95	...	b_1	+13·1	+15·7	+15·3	Indirect comparison; spectrum tremulous; definition bad.
Mar. 13	W.C.	2	3	I	90	r 0·120	b_1	+16·1	−56·2	−34·9	Indirect comparison.

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	15	M.	3	5	I	5	0·085	b_1	+16·4	+ 9·2	+10·4	Indirect comparison; definition variable.
	16	W.C.	2	6	I	120	0·073	b_1	+16·5	−20·4	−25·2	Indirect comparison.
	25	M.	4	8	I	95	0·141	F	+17·5	−52·3	−49·8	Indirect comp.; definition deteriorating.
	30	M.	2	3	I	5	0·123	F	+17·9	−33·3	−34·2	Indirect comparison; definition poor.
Apr.	1	M.	4	13	I	5	0·128	b_1	+18·0	−28·0	−31·1	Indirect comparison.
	7	W.C.	3	6	II	90	0·070	b_1	+18·1	−35·6	−26·3	Indirect comparison; star line seen pretty distinctly.
	8	M.	3	3	II	5	0·072	b_1	+18·1	−23·0	−25·9	Indirect comp.; spectrum very tremulous.
						Mean	Ten-prism Spectroscope			−35·7	−56·7	Weight 8.
							Half-prism Spectroscope			−22·0	−20·5	Weight 60.
							α Hydræ.					
1878 Mar.	13	W.C.	3	4½	I	90	r 0·120	b_1	+ 8·0	+57·6	+24·2	Indirect comparison.
							ε Leonis.					
1878 Apr.	1	M.	2	3	I	5	0·128	b_1	+14·5	−30·4	−36·3	Indirect comparison; star line distinct, but spectrum faint and tremulous.
							Regulus.					
1877 Mar.	5	M.	1	3	2	10	d 1·7	F	+ 5·6	+25·7	+23·7	Indirect comparison.
1878 Mar.	30	M.	2	6	I	5	r 0·073	F	+12·3	+18·5	+18·6	Indirect comparison.
Apr.	11	M.	3	10	I	5	0·027	F	+14·8	+26·6	+21·6	Indirect comparison; definition fair.
						Mean	Ten-prism Spectroscope			+30·7	+32·1	Weight 25.
							Half-prism Spectroscope			+23·6	+20·5	Weight 16.

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Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	r					
γ Leonis.											
1878 Mar. 13	W.C.	3	4	I	90	0·120	b_1	+ 7·9	−13·5	− 3·4	Observations discordant *inter se*.
Apr. 1	M.	2	7	I	5	0·096	b_1	+12·7	−42·5	−40·4	Indirect comparison.
8	M.	3	3	II	5	0·072	b_1	+14·1	−20·8	−23·2	Indirect comparison.
					Mean	Ten-prism Spectroscope			−54·7	−54·0	Weight 8.
						Half-prism Spectroscope			−29·6	−26·1	Weight 14.
β Ursæ Majoris.											
						d d					
1877 Apr. 14	M.	2	6	2	5	3·9 & 3·6	F	+11·9	+22·2	+30·8	Indirect comp.; spectrum fairly defined.
May 2	M.	2	6	2	3	d 3·0	F	+12·7	+41·1	+48·9	Indirect comparison; star spectrum and line well seen.
						Ten-prism Spectroscope			+28·3	+32·2	Weight 21.
β Leonis.											
						r					
1878 Mar. 25	M.	2	4	I	95	0·141	F	+ 4·8	+44·3	+53·4	Indirect comparison; definition poor.
30	M.	3	8	I	5	0·073	F	+ 6·2	+57·0	+54·6	Indirect comparison.
					Mean	Ten-prism Spectroscope			−32·7	−31·0	Weight 2.
						Half-prism Spectroscope			+52·8	+54·2	Weight 12.
γ Ursæ Majoris.											
						d					
1877 May 2	M.	2	6	2	3	3·2	F	+11·8	+24·9	+20·4	Indirect comparison; star spectrum and line well seen.

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							δ Ursæ Majoris.				
1877 May 2	M.	8	4	2	3	3·3	F	+10·8	+ 3·9	+ 0·1	Indirect comp.; spectrum very faint.
							γ Virginis.				
1878 Apr. 11	M.	2	3	1	5	^{t}0·072	F	+ 4·3	+31·8	+26·0	Indirect comparison; definition fair.
							ε Ursæ Majoris.				
1877 May 2	M.	2	6	2	3	^{d}3·3	F	+ 9·6	+ 5·9	+12·3	Indirect comparison.
							α Virginis.				
1878 Mar. 30	M.	4	10	1	5	^{r}0·073	F	− 3·8	− 0·5	− 2·3	Indirect comparison; definition fair.
Apr. 11	M.	2	2	1	5	0·072	F	0·0	+26·8	+25·3	Indirect comparison; definition bad.
May 11	M.	2	4	1	5	...	F	+ 8·7	− 9·3	− 8·7	Indirect comp.; spectrum exceedingly tremulous.
					Mean	Ten-prism Spectroscope			+37·9	+50·9	Weight 4.
						Half-prism Spectroscope			+ 0·7	− 0·5	Weight 16.
							ζ Ursæ Majoris.				
1877 May 2	M.	2	6	2	3	^{d}3·0	F	+ 8·6	+ 5·5	+16·1	Indirect comparison; star spectrum well defined.
							η Boötis.				
1878 Apr. 1	M.	2	4	1	5	^{r}0·128	b_1	− 1·5	−33·4	−34·9	Indirect comparison.

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Arcturus.

Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
					°	d					
1877 Mar. 5	M.	2	7	4	5½	2·5	b_1	− 9·6	−16·2	−50·9	Indirect comparison; lines fairly well defined.
						r					
1878 Mar. 30	M.	4	16	I	5	0·073	F	− 3·3	−17·2	−16·9	Indirect comparison; spectrum steady; definition fair.
Apr. 7	W.C.	2	2	II	90	0·070	b_1	− 1·2	−10·3	− 7·8	Indirect comparison.
8	M.	3	12	II	5	0·072	b_1	− 0·9	−18·3	−20·7	Indirect comp.; definition very good.
				Mean	Ten-prism Spectroscope				−36·2	−45·1	Weight 73.
					Half-prism Spectroscope				−17·2	−17·8	Weight 30.

ϵ_2 *Boötis.*

						r					
1878 Apr. 1	M.	3	9	I	5	0·096	b_1	− 3·4	−26·3	−24·3	Indirect comparison; definition fair.
				Mean	Ten-prism Spectroscope				+ 4·1	− 2·3	Weight 21.
					Half-prism Spectroscope				−26·3	−24·3	Weight 9.

α Coronæ.

1877 Aug. 31	M.	3	8	I	...	...	F	+11·4	+54·4	+67·9	Indirect comparison.
						r					
1878 Mar. 25	M.	3	6	I	95	0·141	F	− 7·6	+54·3	+54·4	Indirect comparison; definition only occasionally good.
				Mean	Ten-prism Spectroscope				+36·0	+39·7	Weight 13.
					Half-prism Spectroscope				+55·4	+62·1	Weight 6.

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							α Ophiuchi.				
1877 Aug. 31	M.	2	8	I	...	...	F	+ 14·3	+ 61·3	+ 89·6	Direct comparison.
1878 Apr. 11	M.	3	3	I	5	0·072 r	F	− 12·7	− 9·0	− 7·5	Indirect comparison; star line very difficult to bisect.
							α Lyræ.				
1876 Dec. 6	M.	2	5	6	6	4·7 d & 2·6 d	F	+ 4·3	− 49·8	− 43·8	Indirect comparison.
						Ten-prism Spectroscope			− 39·1	− 34·6	Weight 92.
							ζ Aquilæ.				
1876 Nov. 17	M.	4	9	2	6	3·0 & 3·9	F	+ 11·9	...	− 66·9	Direct comp.; definition fairly good.
1877 Aug. 23	M.	3	9	I	...	...	F	+ 9·9	− 60·3	− 70·3	Indirect comparison.
							γ Aquilæ.				
1877 Nov. 14	M.	2	5	I	95	...	b_1	+ 14·8	− 3·7	− 2·6	Indirect comparison.
							γ Cygni.				
1877 Nov. 14	M.	2	2½	I	95	....	b_1	+ 10·2	− 27·9	− 40·5	Indirect comparison; definition bad.
						Ten-prism Spectroscope			− 19·7	− 20·4	Weight 24½.
							α Cygni.				
1877 Nov. 14	M.	2	7	I	95	...	b_2 b_1	+ 9·3	− 69·5	− 61·3	Indirect comp.; definition improving.
						Ten-prism Spectroscope			− 40·2	− 41·3	Weight 40.

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Date.	Observer.	No. of Measures.	Weight.	No. of Prisms.	Position-Circle.	Width of Slit.	Line.	Earth's Motion in Miles per Sec.	Concluded Motion of Star. Measured.	Concluded Motion of Star. Estimated.	Remarks.
							ε Cygni.				
1877 Nov. 14	M.	2	4	I	95	d ...	b_1	+ 12·2	+ 24·2	+ 25·7	Indirect comparison; definition poor.
				Ten-prism Spectroscope					+ 11·8	+ 14·9	Weight 3½.
							Moon.				
1878 Jan. 17	M.	4	...	I	5	...	b_1	...	− 0·7	−...	Lines in the spectrum of the Moon rather faint.
Feb. 17	W.C.	1	...	I	0	r 0·156	F	...	− 6·3	0·0	
Mar. 13	W.C.	4	...	I	90	...	b_1	...	− 1·9		
16	W.C.	4	...	I	120	...	b_1	...	− 6·4		
Apr. 8	M.	6	...	II	5	0·072	b_1, b_2, b_4	...	0·0		
11	M.	4	...	I	5	...	F	...	0·0		
May 8	M.	4	...	II	5	...	F	...	+ 0·3	...	The coincidence of the two spectra very perfect.
12	M.	4	...	I	5	...	F	...	+ 0·1		
							Sky Spectrum.				
1878 Apr. 2	M.	15	...	I	5	...	b_1	...	+ 1·8		
8	W.C.	3	...	II	90	...	b_1, b_2, b_4	...	+ 3·0		
8	M.	6	...	II	90	...	b_1, b_2, b_4	...	+ 1·7		
9	M.	8	...	II	5	...	b_1, b_2, b_4	...	+ 0·8		

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As the results with the half-prism spectroscope are quite independent of those formerly obtained, it may be interesting to compare the two sets obtained at Greenwich with those of Dr. Huggins. The following table gives the comparison of the motions found for 51 stars, all of which have been spectroscopically examined at Greenwich. Where no number is set down the result is to be considered as still somewhat uncertain.

The agreement appears to be as satisfactory as can be expected in such delicate observations.

Motions of Stars in the Line of Sight.

\+ denotes recession ; − approach.

The weights are on a scale 1 to 5 for *each* observation.

Star's Name.	Dr. Huggins' Results.	Ten-prism Spectroscope.		Half-prism Spectroscope.	
		Results.	Weight.	Results.	Weight.
α Andromedæ	−	−45	16		
α Arietis				+	6
Aldebaran	+	+	½	+19	22
Capella	+	+24	18	+30	29
β Orionis	+15	+19	8	+21	22½
β Tauri				−	3
γ Orionis				0	15
δ Orionis				+18	5
ε Orionis		−	6	+23	7
ζ Orionis				+	2
α Orionis	+22	+17	11	+22	39
β Aurigæ				+	2
Sirius	+18 to 22	+22	29	+26	15
Castor	+23 to 28	+24	19	+35	11
Procyon	+	+33	24	+22	23
Pollux	−49	−46	8	−21	60
α Hydræ				+	4½
ε Leonis				−	3
Regulus	+12 to 17	+31	25	+22	16
γ Leonis	−	−54	8	−28	14
β Ursæ Majoris	+17 to 21	+30	21		
α Ursæ Majoris	−46 to 60	−	4		
β Leonis	+	−	2	+53	12
γ Ursæ Majoris	+17 to 21	+	6		
δ Ursæ Majoris	+17 to 21	+	4		
γ Virginis				+	3

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Star's Name.	Dr. Huggins' Results.	Ten-prism Spectroscope.		Half-prism Spectroscope.	
		Results.	Weight.	Results.	Weight.
ϵ Ursæ Majoris	+17 to 21	+	6		
α Virginis	+	+	4	0	16
ζ Ursæ Majoris	+ 17 to 21	+	6		
η Ursæ Majoris	+	−32	7		
η Boötis				−	4
Arcturus	−55	−41	73	−18	30
ϵ_2 Boötis		+ 1	21	−	9
α Coronæ	+	+38	13	+58	14
β Herculis		−	3		
α Herculis		−31	5½		
β Draconis		+ 7	1½		
α Ophiuchi		?		?	
γ Draconis		−18	14½		
α Lyræ	−44 to 54	−37	92		
ζ Aquilæ		?		−	9
γ Aquilæ		−	1	−	5
δ Cygni		−23	12		
α Aquilæ		?			
γ Cygni	−	−20	24½	−	2½
α Cygni	−39	−41	40	−	7
ϵ Cygni		+13	3½	+	4
ϵ Pegasi		−24	11½		
Fomalhaut		?			
β Pegasi		+20	10		
α Pegasi	−	−22	16		

Royal Observatory, Greenwich,
1878, *June* 14.

On the Photographs of the Transit of Venus.

By Captain G. L. Tupman, R.M.A.

(*Communicated by the Astronomer Royal*).

The photographs that have been measured were taken with the five Photoheliographs made by Mr. Dallmeyer for the Transit of *Venus* Expeditions on "Patent Plates," 6 inches square, the images of the Sun being very nearly 3·9 inches in diameter. The Dry process of Captain Abney, R.E., described in the *Monthly Notices*, vol. xxxiv., p. 275, was used throughout.

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The measuring instrument, designed by the Astronomer Royal for the purpose, the determination of the errors of graduation of its glass millimeter scale, and the method of obtaining the optical distortion of the Photoheliographs are sufficiently described in vol. xxxvi., p. 178. It was found by an elaborate investigation that the lines of equal distortion were sensibly circles concentric with the centre of the field. The actual correction for distortion for that zone of the field in which the points to be measured generally fall is shown in the following table. By the scale adopted, the distortion is taken as zero at the centre of the field and at the distance of about 47 millimeters from the centre. One millimeter corresponds to about 19·5 seconds of arc.

Distance from centre of field in millimeters	Luxor instrument.	Honolulu instrument.	Rodrigues instrument.	Barnham instrument.	Kerguelen instrument
	mm.	mm.	mm.	mm.	mm.
40	+ ·040	+ ·034	+ ·045	+ ·040	+ ·042
42	+ ·030	+ ·025	+ ·033	+ ·030	+ ·032
44	+ ·019	+ ·015	+ ·021	+ ·020	+ ·021
46	+ ·006	+ ·005	+ ·008	+ ·007	+ ·009
48	− ·006	− ·007	− ·006	− ·006	− ·003
50	− ·021	− ·019	− ·020	− ·020	− ·017
52	− ·037	− ·032	− ·036	− ·036	− ·031

Before commencing the measures of a negative, the position of the line of centres was marked upon the film by a simple mechanical process. This operation was performed independently by Mr. Burton and myself with no sensible difference. In placing the negative in the instrument, the circular carrier was turned about until the line of centres was truly parallel to the direction of the sliding motion of the microscopes.

When the negatives are placed under the microscope with an amplification of only 5 or 6 diameters, the limbs of both planet and Sun, even those which are pretty sharp to the unaided eye, become extremely indistinct, and the act of bisecting the limb with the wire or cross of the micrometer is mere guesswork. The deposit of silver fades off gradually to nothing, and the denser the film the broader is the zone of fading off, and the more uncertain the measures. In many cases the difficulty is aggravated by ruggedness due to atmospheric disturbances, but the smooth and gradual fading off is the chief cause of uncertainty.

There is only *one* really sharp image in the whole collection, including the Indian and Australian contingents, and that is one of Captain Waterhouse's wet plates taken at Roorkee with a Dallmeyer instrument precisely similar to the others.

It should be remarked that in these instruments the artist has attempted to unite the photographic and visual foci on the collodion film. No doubt some sharpness of the photographic

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image was thus sacrificed, but this has little or nothing to do with the unfortunate failure in the application of photography. Each photograph was measured six times by Mr. Burton and six times by me. I was not able to include in my series of measures all the photographs measured by Mr. Burton, for the reason that, when some of them were viewed through the microscope, there was nothing visible to bisect, either from the extreme faintness of the film or from its too gradual fading off. Mr. Burton generally employed a cross of webs, but I preferred a single very fine web, the breadth of which was eliminated in the mean by the mode of bisecting.

It has been suggested that the instrument should possess the power of rotating the Sun's image about a mechanical centre. This would be useful in some cases of ruggedness, but, in my opinion, would make no material difference in the accuracy of measurement. The rotation could only be applied to the limbs of the Sun,* whereas perhaps the greatest difficulty is with the limbs of the planet.

From the measures, corrected for distortion, are obtained the photographic diameters of the Sun and of *Venus*; the former presumably enlarged, the latter diminished by irradiation, in a sensibly equal degree. The sum of the measured diameters in millimeters is compared with the sum of the tabular diameters, subject to errors, for the scale value, and thus every photograph furnishes its own scale. The measured distance of centres *in arc*

$$M = \frac{\text{distance of centres in millimeters} \times \text{sum of tabular diameters}}{\text{sum of measured diameters in millimeters}}.$$

This relation will be true, except when the Sun is near the horizon. As long as the refraction for the centre is sensibly the mean between the refraction at the upper and lower limbs, its effect upon distance of centres will be eliminated.

If dR, dr be the corrections required to the Tabular Semidiameters R, r of the Sun and *Venus* respectively; Δ the Local Tabular distance of centres; and if the True Mean Solar Parallax = Tabular M. S. Parallax $\left(1+\frac{n}{100}\right)$, we have for each photograph, neglecting errors of longitude,

$$\text{True distance of centres} = M + \frac{M}{R+r}(dR+dr) = \Delta + A.\varpi + B.d\text{RA} + C.d\text{NPD}.$$

The coefficients A, B, C, D have been computed for each photograph by the formulæ given in the *Monthly Notices*, vol. XXXV., p. 277. The equations thus obtained were formed into groups of about 10, and the means taken as below:—

* And only with sensibly circular images such as those obtained with a high Sun.

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(1) From Mr. Burton's measures—

			No. of Photos.
Thebes	6″·06 = −″·1413 *n* − ·0422 *d*RA − ·9985 *d*NPD − ·8571 (*d*R + *dr*)		11
	5·82 = −·1629 *n* − ·1188 *d*RA − ·9914 *d*NPD − ·8818 (*d*R + *dr*)		11
	6·40 = −·1773 *n* − ·1782 *d*RA − ·9810 *d*NPD − ·9071 (*d*R + *dr*)		11
Honolulu	8·99 = −·2280 *n* + ·6286 *d*RA − ·7318 *d*NPD − ·9200 (*d*R + *dr*)		11
	8·86 = −·2263 *n* + ·5998 *d*RA − ·7596 *d*NPD − ·9009 (*d*R + *dr*)		11
	8·30 = −·2181 *n* + ·5342 *d*RA − ·8139 *d*NPD − ·8681 (*d*R + *dr*)		10
Rodrigues	5·58 = +·1725 *n* + ·5500 *d*RA − ·8025 *d*NPD − ·9003 (*d*R + *dr*)		11
	6·85 = +·1423 *n* + ·4640 *d*RA − ·8633 *d*NPD − ·8682 (*d*R + *dr*)		10
	4·90 = +·0902 *n* + ·3032 *d*RA − ·9380 *d*NPD − ·8398 (*d*R + *dr*)		10
	5·13 = +·0053 *n* − ·0119 *d*RA − ·9985 *d*NPD − ·8610 (*d*R + *dr*)		11
	3·34 = −·0140 *n* − ·1139 *d*RA − ·9920 *d*NPD − ·8899 (*d*R + *dr*)		11
Burnham	6·70 = +·0300 *n* + ·4803 *d*RA − ·8508 *d*NPD − ·8729 (*d*R + *dr*)		13
Kerguelen	5·34 = +·1669 *n* + ·3042 *d*RA − ·9438 *d*NPD − ·8440 (*d*R + *dr*)		8

From these, considered as of equal weight, we have the three normal equations,

$$
\begin{aligned}
-5''\cdot352 &= +\cdot3159\,n - \cdot0836\,d\mathrm{RA} + \cdot4726\,d\mathrm{NPD} + \cdot5147\,(d\mathrm{R}+dr)\\
+25\cdot446 &= -\cdot0836\,n + 2\cdot0340\,d\mathrm{RA} - 2\cdot7129\,d\mathrm{NPD} - 2\cdot9968\,(d\mathrm{R}+dr)\\
-72\cdot327 &= +\cdot4726\,n - 2\cdot7129\,d\mathrm{RA} + 10\cdot5821\,d\mathrm{NPD} + 10\cdot2255\,(d\mathrm{R}+dr);
\end{aligned}
$$

and their solution is

$$
\begin{aligned}
n &= -7\cdot88 - \cdot233\,(d\mathrm{R}+dr),\\
d\mathrm{RA} &= +5''\cdot38 + \cdot287\,(d\mathrm{R}+dr),\\
d\mathrm{NPD} &= -5\cdot10 - \cdot882\,(d\mathrm{R}+dr);
\end{aligned}
$$

whence the mean Solar Parallax $= 8''\cdot25 - \cdot021\,(d\mathrm{R}+dr)$.

(2) From my own measures—

			No. of Photos.
Thebes	5″·02 = −″·1567 *n* − ·0966 *d*RA − ·9930 *d*NPD − ·8753 (*d*R + *dr*)		12
	5·85 = −·1773 *n* − ·1782 *d*RA − ·9810 *d*NPD − ·9071 (*d*R + *dr*)		11
Honolulu	8·00 = −·2280 *n* + ·6275 *d*RA − ·7329 *d*NPD − ·9182 (*d*R + *dr*)		12
	8·26 = −·2259 *n* + ·5949 *d*RA − ·7641 *d*NPD − ·8974 (*d*R + *dr*)		12
Rodrigues	5·08 = +·1738 *n* + ·5537 *d*RA − ·7996 *d*NPD − ·9016 (*d*R + *dr*)		9
	4·99 = +·1444 *n* + ·4704 *d*RA − ·8595 *d*NPD − ·8680 (*d*R + *dr*)		6
	4·56 = +·1058 *n* + ·3556 *d*RA − ·9223 *d*NPD − ·8407 (*d*R + *dr*)		8
	4·21 = +·0036 *n* − ·0197 *d*RA − ·9986 *d*NPD − ·8620 (*d*R + *dr*)		10
	2·59 = −·0141 *n* − ·1142 *d*RA − ·9921 *d*NPD − ·8890 (*d*R + *dr*)		9
Burnham	5·77 = +·0179 *n* + ·5699 *d*RA − ·7862 *d*NPD − ·9072 (*d*R + *dr*)		3
	6·05 = +·0336 *n* + ·4494 *d*RA − ·8702 *d*NPD − ·8619 (*d*R + *dr*)		10

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From these, weighted as the square root of the number of photographs, are obtained the normal equations,

$$\begin{aligned}
-46''\cdot84 &= +2''\cdot377\, n - 1''\cdot029\, d\mathrm{RA} + 4''\cdot388\, d\mathrm{NPD} + 5''\cdot231\,(d\mathrm{R}+dr)\\
+188''\cdot09 &= -1''\cdot029\, n + 17''\cdot680\, d\mathrm{RA} - 21''\cdot002\, d\mathrm{NPD} - 24''\cdot232\,(d\mathrm{R}+dr)\\
-496''\cdot40 &= +4''\cdot838\, n - 21''\cdot002\, d\mathrm{RA} + 81''\cdot112\, d\mathrm{NPD} + 79''\cdot868\,(d\mathrm{R}+dr);
\end{aligned}$$

and their solution is

$$\begin{aligned}
n &= -9''\cdot70 - ''\cdot445\,(d\mathrm{R}+dr),\\
d\mathrm{RA} &= +5''\cdot04 + ''\cdot298\,(d\mathrm{R}+dr);\\
d\mathrm{NPD} &= -3''\cdot36 - ''\cdot881\,(d\mathrm{R}+dr);
\end{aligned}$$

whence the Mean Solar Parallax $= 8''\cdot08 - ''\cdot040\,(d\mathrm{R}+dr)$.

These values of the parallax being quite inadmissible, the Astronomer Royal suggested that the tolerably well-known values of n, $d\mathrm{RA}$, and $d\mathrm{NPD}$, obtained from the contact observations, should be substituted in the equations, so that the outstanding quantities might appear as errors of the distance of centres or of the photographic semidiameters. Accordingly, n being taken $-1''\cdot117$, corresponding to the mean solar parallax $8''\cdot85$, $d\mathrm{RA} = +5''\cdot81$, $d\mathrm{NPD} = -5''\cdot33$, the following equations result:—

Station.	From Burton's measures.	No. of Photos.	From Tupman's measures.	No. of Photos.
Thebes	$''\cdot857\,(d\mathrm{R}+dr) = -''\cdot83$	11		
	$''\cdot882\,(d\mathrm{R}+dr) = -1''\cdot04$	11	$''\cdot875\,(d\mathrm{R}+dr) = -''\cdot11$	12
	$''\cdot907\,(d\mathrm{R}+dr) = -2''\cdot01$	11	$''\cdot907\,(d\mathrm{R}+dr) = -1''\cdot46$	11
Honolulu	$''\cdot920\,(d\mathrm{R}+dr) = -1''\cdot18$	11		
	$''\cdot901\,(d\mathrm{R}+dr) = -1''\cdot07$	11	$''\cdot918\,(d\mathrm{R}+dr) = -''\cdot19$	12
	$''\cdot868\,(d\mathrm{R}+dr) = -''\cdot61$	10	$''\cdot897\,(d\mathrm{R}+dr) = -''\cdot48$	12
Rodrigues	$''\cdot900\,(d\mathrm{R}+dr) = +1''\cdot07$	11	$''\cdot902\,(d\mathrm{R}+dr) = +2''\cdot20$	9
	$''\cdot868\,(d\mathrm{R}+dr) = +''\cdot29$	10	$''\cdot868\,(d\mathrm{R}+dr) = +2''\cdot16$	6
	$''\cdot840\,(d\mathrm{R}+dr) = +1''\cdot76$	10	$''\cdot841\,(d\mathrm{R}+dr) = +2''\cdot30$	8
	$''\cdot861\,(d\mathrm{R}+dr) = +''\cdot12$	11	$''\cdot862\,(d\mathrm{R}+dr) = +1''\cdot09$	10
	$''\cdot890\,(d\mathrm{R}+dr) = +1''\cdot30$	11	$''\cdot889\,(d\mathrm{R}+dr) = +2''\cdot05$	9
Burnham	$''\cdot873\,(d\mathrm{R}+dr) = +''\cdot59$	13	$''\cdot907\,(d\mathrm{R}+dr) = +1''\cdot71$	3
			$''\cdot862\,(d\mathrm{R}+dr) = +1''\cdot19$	10
Kerguelen	$''\cdot844\,(d\mathrm{R}+dr) = +1''\cdot27$	8		0

The discordances here shown are of three kinds:—

1° Taking the Rodrigues results by themselves, as they are the most numerous and the photographs present the sharpest limbs, Mr. Burton's measures show a discordance of $1''\cdot6$ between the means of two contiguous groups. My measures also have

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the greatest discordance, 1″·2, between the same groups. These discordances are obviously due to the inherent defects of the photographic images, and are of such magnitude as to forbid the employment of the measures in the determination of the solar parallax.

2° There is a curious systematic difference between Mr. Burton and myself amounting to nearly 1″. Mr. Burton invariably placed the negative in the measuring instrument with the planet on the left-hand side of the Sun's centre, whereas I always placed it with the planet on the right-hand side. It is difficult to conceive, however, in what manner this can have affected our appreciation of the diameters or of the distance of centres.

3° There is a systematic difference between the Northern and the Southern stations of a very puzzling character, affecting the distance of centres to the extent of about 2″, or about one-twelfth part of the total relative parallactic displacement of the planet referred to the centre of the Sun. No cause for this has at present been suggested. These discordances support the decision of the American Commission that the photographic diameter of the Sun cannot be relied on when accuracy is required.

Additions and Corrections to Captain Tupman's Paper in the Monthly Notices for June 1878.

Page 449, line 18. The normal equations are

$$+31{\cdot}0000\,i - 0{\cdot}0787\,n = -8{\cdot}3328'',$$
$$-\ 0{\cdot}0787\,i + 0{\cdot}6112\,n = -0{\cdot}5744''.$$

Page 450, line 22. The normal equations are

$$+28{\cdot}0000\,i - 0{\cdot}2055\,n = -1{\cdot}7466'',$$
$$-\ 0{\cdot}2055\,i + 0{\cdot}6565\,n = -0{\cdot}7581'';$$

whence

$$i = -0{\cdot}177'', \qquad n = -1{\cdot}071''.$$

Page 453, line 2. The normal equations are

$$+55{\cdot}0000\,e + 0{\cdot}9377\,n = -0{\cdot}6794'',$$
$$+\ 0{\cdot}9377\,e + 1{\cdot}0793\,n = -1{\cdot}2416''.$$

Page 454, line 11. The normal equations are

$$+48{\cdot}0000\,e + 1{\cdot}5013\,n = -3{\cdot}7696'',$$
$$+\ 1{\cdot}5013\,e + 0{\cdot}9343\,n = -0{\cdot}6729''.$$

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Spectroscopic Observations of the Eclipse of the Moon, 1878, August 12, made at the Royal Observatory, Greenwich.

(*Communicated by the Astronomer Royal*).

The following observations were made by Mr. Maunder, with the Great Equatoreal and Single Prism Spectroscope:—

The Moon, as viewed in the finder of the Great Equatoreal, appeared to have the eclipsed part completely cut out from it, the shadow being so dense and black that the outline of the Moon's limb could not be traced under it, even in imagination; nor could any features be made out on the eclipsed part, except at the very edge of the shadow. As viewed in the Single Prism Spectroscope, there was by no means the same difference in the amount of apparent brilliancy. The spectrum of the bright part was about three times as bright as that of the part in shadow, and could not be traced much further either way, the bright part being visible from Fraunhofer's cluster B to midway between *h* and H; the eclipsed part from about wave-length 6450 tenth-metres ±, near Band *β* of the Brewster-Gladstone spectrum, to *h*. The spectrum of the eclipsed part presented a great contrast to its appearance during the total eclipse of 1877, August 23, in the almost total absence of the telluric bands; Band *δ*, which was so dark and broad on that occasion, being now scarcely visible; and the only other one seen was one just suspected near Band *o*. The only other evidence of atmospheric absorption was a general absorption beyond D to the red, and beyond G to the violet.

Royal Observatory, Greenwich,
1878, *August* 31.

The Proper Motions of certain Stars in the Greenwich Seven-Year Catalogue for 1864. By A. W. Downing, B.A.

Having occasion lately to refer to the proper motions of stars in the first four hours of right ascension, I was surprised to find that there were still so many of the stars observed by Bradley whose proper motions have not been determined by comparison with accurate modern places; so that astronomers still use, in many cases, the untrustworthy proper motions of the British Association Catalogue.

The present paper is an attempt to partially supply, for stars in the first four hours of right ascension, this deficiency in our knowledge of accurate proper motions. It contains the proper motions of the stars, within the above-mentioned limits of right ascension, which are common to Bradley's Catalogue in Bessel's *Fundamenta Astronomiæ* and the Greenwich Seven-Year Catalogue for 1864, the quantities of proper motion being deduced

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from a comparison of the places given in these catalogues. I have, of course, omitted the Bradley stars in the latter catalogue whose proper motions have been determined by Main or Stone, and I have also omitted those which appear in the new Nine-Year Catalogue for 1872, so that the places of all in my list must depend, at least for some time to come, on the Seven-Year Catalogue for 1864.

The method of computation which I have adopted is that explained by Mr. Main in the 19th volume of the *Memoirs* of this Society, viz., if p_1, p_2, and p_3 be the annual precessions, in R.A. or N.P.D., for any star at equidistant epochs, the mean annual precession $= \frac{1}{6}(p_1 + 4p_2 + p_3)$; and I have applied the necessary corrections to Bessel's precessions as given in the *Fundamenta* to reduce them to precessions deduced from Peters' Constants. Then, as Bessel has given the precessions of each star for the epochs 1755 and 1800, it is easy from the latter to compute the precession for the middle year between 1755 and the mean year of observation of the star given in the Greenwich Catalogue, and from the precession given in the latter catalogue to compute it for the mean year of observation, and thus obtain three precessions at equidistant epochs, and thence, by means of the formula given above, to find the mean annual precession.

In computing the mean annual motions I have been careful to correct the places in the Seven-Year Catalogue for the quantity of proper motion applied to the observed place to reduce it to the mean epoch of the catalogue, but the places in the *Fundamenta* have been used without any correction whatever. Of course it is very easy to correct the resulting proper motions for any correction that may be applied to the *Fundamenta*, as it is simply $-\frac{\Delta\alpha}{109}$ or $-\frac{\Delta\delta}{109}$, where $\Delta\alpha$ and $\Delta\delta$ are the corrections to the R.A. and N.P.D. respectively.

Having found the mean annual precession and mean annual motion, then in all cases

Proper motion = Mean annual motion − Mean annual precession.

As none of the stars whose proper motions are given in this paper are very near the pole, the same method has been used for all.

Unfortunately there are several instances in which a star has been observed by Bradley in one element only, and some in which the place in the *Fundamenta* depends on a single observation. In this latter case I have always marked the resulting proper motion with an asterisk.

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No.	Name of Star.	Approx. R.A. 1864. h	m	Mean Precession in R.A. s	Mean Annual Motion. s	Proper Motion in R.A. s	Mean Precession in N.P.D. "	Mean Annual Motion. "	Proper Motion in N.P.D. "
1	4 Ceti	0	1	+3·0717	+3·0705	−0·001	−20·058	−20·077	−0·02
2	5 Ceti	0	1	3·0715	3·0670	−0·005	20·059	20·058	0·00
3	23 Andromedæ	0	6	3·0894	3·0767	−0·013	20·057	19·919	+0·14
4	7 Ceti	0	8	3·0604	3·0586	−0·002	20·054	19·981	+0·07
5	36 Piscium	0	9	3·0755	3·0716	−0·004	20·050	20·022	+0·03
6	37 Piscium	0	10	3·0894	3·0784	−0·002			
7	38 Piscium, 2nd star	0	10	3·0772	3·0788	+0·002	20·048	20·128	−0·08
8	39 Piscium	0	11	3·0843	3·0998	+0·016	20·047	20·023	+0·02
9	42 Piscium	0	15	3·0873	3·0906	+0·003	20·029	20·037	−0·01
10	9 Ceti	0	16	3·0530	3·0780	+0·025	20·026	20·082	−0·06
11	Bradley 27	0	19	3·1038	3·1016	−0·002*			
12	Bradley 30	0	20	3·0986	3·1053	+0·007*	20·000	20·008	−0·01*
13	46 Piscium	0	21	3·1063	3·1025	−0·004	19·997	19·983	+0·01
14	47 Piscium	0	21	3·1032	3·1110	+0·008	19·996	20·027	−0·03
15	48 Piscium	0	21	3·1005	3·1000	−0·001	19·995	19·982	+0·01
16	49 Piscium	0	24	3·1038	3·0978	−0·006	19·975	19·996	−0·02
17	Bradley 41	0	25	3·1049	3·1026	−0·002	19·970	19·943	+0·03
18	54 Piscium	0	32	3·1344	3·0991	−0·035	19·892	19·518	+0·37
19	55 Piscium	0	33	3·1360	3·1365	+0·001	19·888	19·850	+0·04
20	32 Andromedæ	0	34	3·2138	3·2138	0·000	19·877	19·876	0·00

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21	Bradley 64	0	35	3·2242	3·2209	−0·003*			
22	Bradley 68	0	37	3·2818	3·2761	−0·006			
23	18 Ceti	0	39	3·0194	3·0130	−0·006	19·813	19·593	+0·22
24	57 Piscium	0	39	3·1260	3·1234	−0·003	19·802	19·753	+0·05
25	61 Piscium	0	41	3·1507	3·1589	+0·008	19·785	19·771	+0·01
26	62 Piscium	0	41	3·0955	3·0987	+0·003	19·776	19·765	+0·01
27	64 Piscium	0	42	3·1357	3·1324	−0·003	19·768	19·554	+0·21
28	6 Piscium	0	43	3·1870	3·1913	+0·004	19·756	19·736	+0·02
29	ϕ^2 Ceti	0	43	3·0225	3·0046	−0·018	19·744	19·504	+0·24
30	66 Piscium	0	47	3·1660	3·1541	−0·002	19·681	19·675	+0·01
31	21 Ceti	0	47	3·0264	3·0262	0·000	19·678	19·618	+0·06
32	ϕ^3 Ceti	0	49	3·0123	3·0086	−0·003	19·646	19·629	+0·02
33	ϕ^1 Ceti	0	52	3·0084	3·0036	−0·005	19·597	19·562	+0·04
34	25 Ceti	0	56	3·0394	3·0287	−0·011	19·515	19·410	+0·11
35	Bradley 112	0	56	3·4823	3·4648	−0·018*			
36	Bradley 114	0	57	3·7376	3·7397	+0·002*			
37	ψ^1 Piscium	0	58	3·1907	3·1938	+0·003	19·473	19·426	+0·05
38	Bradley 122	0	58	3·1907	3·1928	+0·002	19·473	19·426	+0·05
39	27 Ceti	0	59	3·0081	3·0038	−0·004	19·460	19·424	+0·04
40	28 Ceti	0	59	3·0083	3·0077	−0·001	19·450	19·470	−0·02
41	78 Piscium	1	1	3·2710	3·2836	+0·013	19·429	19·381	+0·05
42	ψ^2 Piscium	1	1	+3·1908	+3·1947	+0·004	−19·423	−19·327	+0·10

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No.	Name of Star.	Approx. R.A. 1864. h	m	Mean Precession in R.A. s	Mean Annual Motion. s	Proper Motion in R.A. s	Mean Precession in N.P.D. "	Mean Annual Motion. "	Proper Motion in N.P.D. "
43	30 Ceti	1	1	+3·0071	+3·0159	+0·009	−19·414	−19·409	+0·01
44	29 Ceti	1	1	3·0770	3·0833	+0·006	19·416	18·952	+0·46
45	44 Andromedæ	1	3	3·3709	3·3578	−0·013	19·383	19·320	+0·06
46	32 Cassiopeiæ	1	3	3·7761	3·7743	−0·002	19·385	19·371	+0·01
47	32 Ceti	1	3	3·0099	3·0062	−0·004	19·359	19·299	+0·06
48	45 Andromedæ	1	4	3·3317	3·3287	−0·003	19·361	19·365	0·00*
49	*g* Piscium	1	4	3·2768	3·2748	−0·002	19·359	19·349	+0·01*
50	Bradley 153	1	6	3·2715	3·2737	+0·002			
51	35 Ceti	1	6	3·0803	3·0752	−0·005			
52	36 Ceti	1	6	3·0212	3·0187	−0·003	19·298	19·277	+0·02
53	ϕ Piscium	1	6	3·2309	3·2297	−0·001	19·292	19·259	+0·03
54	87 Piscium	1	7	3·1709	3·1656	−0·005	19·279	19·251	+0·03
55	88 Piscium	1	8	3·1103	3·1071	−0·003	19·260	19·225	+0·04
56	39 Ceti	1	10	3·0475	3·0386	−0·009	19·208	19·138	+0·07
57	41 Ceti	1	11	3·0113	3·0101	−0·001	19·176	19·117	+0·06
58	Bradley 166	1	12	4·8594	4·8585	−0·001			
59	35 Cassiopeiæ, 2nd star	1	12	3·8687	3·8758	+0·007	19·166	19·130	+0·04*
60	*l* Piscium	1	14	3·2847	3·2858	+0·001	19·111	19·020	+0·09
61	43 Ceti	1	16	3·0605	3·0578	−0·003	19·052	19·038	+0·01
62	47 Andromedæ	1	16	3·3833	3·3887	+0·005	19·052	19·030	+0·02

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63	92 Piscium	1	17	3·1989	3·2009	+0·002	19·030	18·968	+0·06
64	46 Ceti	1	19	2·9492	2·9489	0·000	18·956	18·957	0·00
65	Bradley 191	1	19	3·0603	3·0599	0·000*			
66	47 Ceti	1	20	2·9601	2·9594	−0·001	18·923	18·909	+0·01
67	95 Piscium	1	21	3·1050	3·1004	−0·005	18·914	18·762	+0·15
68	97 Piscium	1	23	3·2136	3·2152	+0·002			
69	48 Ceti	1	23	2·8794	2·8809	+0·002			
70	Bradley 204	1	25	3·2048	3·2107	+0·006*			
71	Bradley 195	1	26	5·8213	5·8126	−0·009*			
72	100 Piscium	1	28	3·1702	3·1670	−0·003	18·702	18·687	+0·02
73	49 Ceti	1	28	2·9256	2·9282	+0·003	18·684	18·672	+0·01
74	50 Ceti	1	29	2·9259	2·9254	−0·001	18·640	18·641	0·00
75	χ Andromedæ	1	31	3·5463	3·5418	−0·004*	18·597	18·591	+0·01
76	42 Cassiopeiæ	1	32	4·4394	4·4382	+0·008	18·580	18·574	+0·01
77	Bradley 225	1	34	3·3588	3·3565	−0·002	18·499	18·470	+0·03
78	Bradley 236	1	41	3·2399	3·2261	−0·005			
79	Bradley 240	1	44	3·7635	3·7610	−0·003*			
80	55 Andromedæ	1	45	3·5506	3·5471	−0·004	18·104	18·087	+0·02
81	7 Arietis	1	48	3·3183	3·3161	−0·002	17·979	17·970	+0·01
82	47 Cassiopeiæ	1	52	5·5428	5·5676	+0·025*	17·928	17·874	+0·05
83	49 Cassiopeiæ	1	53	5·3672	5·3545	−0·013*	17·874	17·887	−0·01
84	52 Cassiopeiæ	1	53	+4·3253	+4·3230	−0·002	−17·833	−17·815	+0·02

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No.	Name of Star.	Approx. R.A. 1864. h	m	Mean Precession in R.A. s	Mean Annual Motion. s	Proper Motion in R.A. s	Mean Precession in N.P.D. "	Mean Annual Motion. "	Proper Motion in N.P.D. "
85	57 Ceti	1	53	+2·8229	+2·8212	−0·002	−17·759	−17·749	+0·01
86	51 Cassiopeiæ	1	54	5·1640	5·1490	−0·015*	17·825	17·793	+0·03*
87	10 Arietis	1	56	3·3662	3·3757	+0·010	17·673	17·679	−0·01
88	60 Ceti	1	56	3·0622	3·0657	+0·004	17·650	17·662	−0·01
89	Bradley 279	1	57	3·3679	3·3675	0·000*			
90	61 Ceti	1	57	3·0570	3·0598	+0·003	17·624	17·545	+0·08
91	Bradley 284	1	59	3·3724	3·3714	−0·001*			
92	11 Arietis	1	59	3·3710	3·3674	−0·004	17·537	17·506	+0·03
93	58 Andromedæ	2	0	3·5633	3·5734	+0·010	17·495	17·435	+0·06
94	14 Arietis	2	2	3·3801	3·3839	+0·004	17·429	17·383	+0·05
95	62 Ceti	2	2	3·0334	3·0272	−0·006	17·391	17·362	+0·03
96	59 Andromedæ	2	3	3·5937	3·5909	−0·003	17·397	17·370	+0·03
97	Bradley 294	2	3	3·5937	3·5910	−0·003			
98	5 Trianguli	2	3	3·4681	3·4700	+0·002*	17·354	17·330	+0·02*
99	δ Andromedæ	2	5	3·7090	3·7023	−0·007	17·309	17·281	+0·03
100	63 Ceti	2	5	3·0388	3·0384	0·000*	17·285	17·230	+0·06
101	66 Ceti	2	6	3·0318	3·0598	+0·028	17·231	17·165	+0·07
102	Bradley 309	2	6	3·3035	3·3035	0·000			
103	20 Arietis	2	8	3·3929	3·4043	+0·011	17·152	17·076	+0·08
104	21 Arietis	2	8	3·3820	3·3748	−0·007	17·151	17·058	+0·09

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105	8 Persei	2	8	4·1383	4·1388	+0·001*	17·158	17·148	+0·01
106	γ Trianguli	2	9	3·5246	3·5282	+0·004	17·097	17·057	+0·04
107	23 Arietis	2	12	3·3135	3·3106	−0·003	16·982	16·859	+0·12
108	Bradley 325	2	12	3·8186	3·8195	+0·001	16·985	16·978	+0·01
109	63 Andromedæ	2	12	3·8975	3·8966	−0·001	16·985	16·952	+0·03
110	70 Ceti	2	15	3·0487	3·0454	−0·003	16·796	16·727	+0·07
111	64 Andromedæ	2	15	3·9129	3·9127	0·000	16·824	16·779	+0·05
112	10 Persei	2	16	4·1409	4·1388	−0·002	16·820	16·828	−0·01
113	Bradley 336	2	17	3·1934	3·1890	−0·004*			
114	71 Ceti	2	18	3·0236	3·0238	0·000	16·657	16·673	−0·02
115	66 Andromedæ	2	19	3·9492	3·9500	+0·001	16·665	16·557	+0·11
116	ρ Ceti	2	19	2·8954	2·8930	−0·002	16·589	16·572	+0·02
117	11 Trianguli	2	19	3·5203	3·5177	−0·003	16·614	16·582	+0·03
118	25 Arietis	2	20	3·1952	3·1746	−0·021	16·564	16·344	+0·22
119	13 Trianguli	2	21	3·4915	3·4846	−0·007	16·543	16·595	−0·05
120	26 Arietis	2	23	3·3353	3·3379	+0·003	16·431	16·396	+0·04
121	σ Ceti	2	26	3·8457	3·8395	−0·006	16·273	16·175	+0·10
122	Bradley 355	2	26	3·3255	3·3292	+0·004	16·277	16·292	−0·02*
123	77 Ceti	2	28	2·9498	2·9527	+0·003	16·157	16·066	+0·09
124	80 Ceti	2	29	2·9493	2·9408	−0·009	16·089	16·019	+0·07
125	Bradley 358	2	31	+4·9705	+4·9980	+0·028*	−16·109	−16·089	+0·02

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No.	Name of Star.	Approx. R.A. 1864. h m	Mean Precession in R.A. s	Mean Annual Motion. s	Proper Motion in R.A. s	Mean Precession in N.P.D. "	Mean Annual Motion. "	Proper Motion in N.P.D. "
126	81 Ceti	2 31	+3·0116	+3·0140	+0·002	−16·010	−15·971	+0·04
127	11 Persei	2 33	4·2012	4·2061	+0·005	15·932	15·913	+0·02
128	84 Ceti	2 34	3·0495	3·0533	+0·004*	15·833	15·700	+0·13
129	Bradley 379	2 35	3·2121	3·2106	−0·002*			
130	Bradley 391	2 41	3·4562	3·4600	+0·004	15·490	15·480	+0·01
131	Bradley 403	2 45	3·1578	3·1589	+0·001*			
132	ρ^1 Arietis	2 47	3·3390	3·3402	+0·001	15·122	15·100	+0·02
133	Bradley 396	2 51	8·4248	8·3936	−0·031*	15·171	15·173	0·00
134	Bradley 416	2 53	3·7105	3·7141	+0·004*	14·820	14·780	+0·04*
135	50 Arietis	2 53	3·3513	3·3479	−0·003			
136	7 Eridani	2 54	3·0133	3·0116	−0·002*	14·711	14·732	−0·02
137	ρ^1 Eridani	2 54	2·9362	2·9411	+0·005	14·677	14·588	+0·09
138	93 Ceti	2 55	3·1286	3·1233	−0·005	14·642	14·639	0·00
139	ρ^2 Eridani	2 56	2·9345	2·9342	0·000	14·586	14·571	+0·02
140	52 Arietis	2 57	3·4914	3·4884	−0·003	14·530	14·506	+0·02
141	54 Arietis	3 1	3·3761	3·3756	−0·001	14·331	14·346	−0·02
142	Bradley 444	3 2	3·5340	3·5335	−0·001*			

No. 142. The R.A. of the *Fundamenta* has been increased 1′.

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143	59 Arietis	3	12	3·5573	3·5538	−0·004	13·639	13·549	+0·09
144	κ^1 Ceti	3	12	3·1169	3·1331	+0·016	13·587	13·605	−0·02
145	l Persei	3	12	3·9745	3·9676	−0·007	13·626	13·607	+0·02
146	60 Arietis	3	12	3·5288	3·5288	0·000	13·600	13·496	+0·10
147	62 Arietis	3	14	3·5734	3·5738	0·000	13·497	13·457	+0·04
148	Bradley 475	3	17	3·4619	3·4663	+0·004*			
149	Bradley 480	3	21	4·1757	4·1800	+0·004	13·072	13·021	+0·05
150	Bradley 483	3	23	4·1790	4·1787	0·000			
151	36 Persei	3	23	4·1077	4·1030	−0·005	12·932	12·849	+0·08
152	Bradley 490	3	27	3·7030	3·7047	+0·002			
153	Bradley 492	3	27	3·6963	3·6921	−0·004			
154	Bradley 496	3	30	3·0704	3·0739	+0·004	12·413	12·232	+0·18
155	21 Eridani	3	32	2·9551	2·9500	−0·005	12·234	12·004	+0·23
156	o Persei	3	34	3·7703	3·7700	0·000	12·181	12·164	+0·02
157	22 Eridani	3	34	2·9623	2·9601	−0·002	12·124	12·115	+0·01
158	24 Eridani	3	37	3·0377	3·0343	−0·003	11·867	11·853	+0·01
159	21 Tauri	3	38	3·5492	3·5509	+0·002	11·892	11·839	+0·05
160	22 Tauri	3	38	3·5490	3·5490	0·000	11·882	11·820	+0·06*
161	π^1 Tauri	3	38	3·1741	3·1744	0·000*	11·816	11·788	+0·03
162	π Eridani	3	40	2·8262	2·8256	−0·001*	11·705	11·752	−0·05
163	Bradley 522	3	40	+3·5281	+3·5272	−0·001	−11·709	−11·648	+0·06

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No.	Name of Star.	Approx. R.A. 1864. h m	Mean Precession in R.A. s	Mean Annual Motion. s	Proper Motion in R.A. s	Mean Precession in N.P.D. "	Mean Annual Motion. "	Proper Motion in N.P.D. "
164	Bradley 523	3 40	+3·5491	+3·5496	+0·001*			
165	26 Tauri	3 41	3·5388	3·5379	−0·001			
166	*n* Persei	3 41	3·7639	3·7604	−0·004	−11·669	−11·670	0·00
167	ω^2 Tauri	3 45	3·1859	3·1833	−0·003	11·369	11·346	+0·02
168	29 Eridani	3 46	2·9620	2·9597	−0·002			
169	30 Eridani	3 46	2·9560	2·9544	−0·002	11·264	11·241	+0·02
170	*A* Persei	3 47	4·3932	4·4011	+0·008	11·316	11·177	+0·14
171	τ^6 Eridani	3 48	2·5472	2·5490	+0·002	11·096	11·076	+0·02
172	Bradley 545	3 53	3·5414	3·5417	0·000	10·798	10·744	+0·05
173	τ^5 Eridani	3 54	2·5529	2·5528	0·000	10·641	10·646	−0·01
174	40 Tauri	3 57	3·1687	3·1658	−0·003	10·504	10·476	+0·03
175	49 Persei	3 59	3·9412	3·9321	−0·009	10·348	10·186	+0·16
176	50 Persei	3 59	+3·9515	+3·9660	+0·014	−10·328	−10·129	+0·20

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Letter from Professor Watson to Mr. Hind, Superintendent of the "Nautical Almanac."

I doubt not that you will be interested to receive more precise information about the observations which I made on July 29 than has yet been published.

Upon my return from the Eclipse Expedition, in order to be able to answer numerous inquiries which were addressed to me, I made an approximate reading of the circles upon which I had marked the positions observed, and the resulting places of two new stars which I observed were immediately communicated to several astronomers in this country and in Europe, and also to the *American Journal of Science*. As soon as I had the leisure, I made a more careful determination, and I noticed, upon revolving the alidade of the reading-circle, that the circle to be read had a very considerable excentricity, as it had been hurriedly mounted. Since there would, in any event, be an error of excentricity to be determined in changing the hour-circle from the instrument to the reading-circle, I did not remount it, but determined the necessary corrections by means of the four readings upon the Sun.

I have made ten readings upon each mark, backward and forward, so that each reading is an independent determination. The results derived from the mean of these readings are shown by the following:—

Chronometer Times. By Newcomb's. h	m	s	By Watson's. h	m	s	Washington M.T. h	m	s	Object observed.	Circle Reading. °	′	″
			4	39	50	5	7	31	Sun	165	31·6	±0·7
10	29	15				5	16	37	(*a*)	161	1·6	±0·4
10	30	24				5	17	46	(*b*)	156	7·5	±0·5
			4	55	10	5	22	51	Sun	161	38·3	±0·5
			5	4	50	5	32	31	Sun	159	7·0	±0·3
			5	46	55	6	14	36	Sun	148	21·7	±0·7

By comparing the mean of the second and third readings upon the Sun with the extreme readings, I have obtained the following expression for the correction for excentricity:—

$$R_c = R + 103''.7 \sin (R + 63^\circ.2);$$

so that we have

Washington M.T. h	m	s	Object observed.	Corrected Circle Reading. °	′	″
5	7	31	Sun	164	13·7	
5	16	37	(*a*)	159	49·3	
5	17	46	(*b*)	155	1·8	
5	22	51	Sun	160	25·2	
5	32	31	Sun	157	57·2	
6	14	36	Sun	147	27·4	

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From these I derive the following differences of right ascension between (*a*) and (*b*) and the Sun:—

From	(*a*)−⊙ Δα	(*b*)−⊙ Δα
	m s	m s
S_1 =	−8 31·6	−26 32·6
S_2 =	−8 37·6	−26 38·2
S_3 =	−8 25·6	−26 26·6
S_4 =	−8 31·5	−26 32·4

The differences of declination previously determined were respectively −0° 22′ and −0° 35′; and hence we have

Washington M.T.			Planet−⊙ Δα	Δδ	Apparent α	δ
	h m s		m s	° ′	h m s	° ′
1878 July 29	5 16 37	(*a*)	− 8 32	−0 22	8 27 24	+18 16
29	5 17 46	(*b*)	−26 32	−0 35	8 9 24	+18 3

The magnitude of (*a*) was 4 to 4½; that of (*b*) about 3½. They were probably really brighter because the illumination of the sky was not considered in the estimates.

Before I came to reduce the observations I thought that the star (*b*) might possibly have been *ζ Cancri*, because I did not see the latter also. The sweep was not extended beyond (*b*), and just as I had recorded its place, the Sun came out. The time available for the observations was so short that it was not possible to get more data than I have given above. I did see *θ Cancri* as well as (*a*), and the place of the latter is sure. I consider that of (*b*) to be also sure, and hence that it is a new star.

Ann Arbor, 1878, *Sept.* 3.

Note on Brightness.

By R. A. Proctor, Esq., Hon. Fellow, King's College, London.

I regret that I should have again misunderstood Mr. Stone. I thought he referred only to the light-gathering power of the 26-inch telescope: for he gave no reasons, and I should have expected reasons, for a proposition certainly not self-evident. He asked simply why the satellites of *Mars* can be seen with large telescopes, though invisible with small ones. I think it unlikely that the visibility of the satellites of *Mars* with large telescopes depends at all on the smaller size of the diffraction image. The quantity of light forming the image is the chief point to be considered (I observe that Prof. Newcomb, in estimating the size of

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the satellites, considers this point only). I believe it is the only point the eye can appreciate.* In considering the separating powers of telescopes, the size of the diffraction image is of course an all-important point; but it can scarcely affect the space-penetrating power.

1878, *August 29.*

Note upon the Proper Motion of 5 Serpentis.

By Professor T. H. Safford.

The right ascension of this star as given in Bessel's Bradley is about $20'' = 1^s.33$ too large. By a new reduction of Bradley's three observations, I find for 1755·0, but without proper motion, using Newcomb's standard right ascensions,

			Mean R.A. h m s
1751	Jan. 7	Old Style	15 6 49·56
	May 26	,,	49·46
	June 3	,,	49·94
Mean (1751·3)			15 6 49·65
The *Fundamenta* gives			15 6 50·90

D'Agelet's one observation as reduced by Dr. Gould,

	h m s
1800·0 (1784·4)	15 9 5·7,

needs a correction of $+0^s.74$. The time-stars for this date, June 5, 1784, are *Capella* and *Rigel*, and passed but a few minutes after the Sun. Nineteen small stars observed in the evening give corrections, when compared with their places from Bradley, brought up with Mädler's proper motion, of which one only is negative, and the mean of all is $+0^s.737 \pm 0^s.063$, showing that (probably) the instrument was exposed to the Sun's heat at noon and changed its position during the afternoon.

The correction is confirmed by five other stars, whose right ascensions were found in Bailey's Lalande.

With these corrections, and not without, the right ascensions can be made to agree well with a formula including position for

* In the sense of distinguishing between the intensities of the impressions produced on the optic nerves by two diffraction images, unequal in size but both very small, and equal as regards quantity of light.

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1875 and proper motion. As reduced to the equinox and equator of 1875, they are as follows :—

		R.A. h	m	s		C−O s
Bradley	1751·3	15	12	52·95	3 obs.	−0·02
D'Agelet	1784·4			53·62	1 „	+0·14
Lalande	1797·9			54·14	2 „	−0·05
Argelander	1830·			54·90	12 „	+0·10
Robinson	1831·67			54·92	4 „	+0·02
Challis	1836·4			55·07	3 „	−0·01
Airy	1845·			55·38	3 „	−0·11
Wagner	1849·4			55·39	4 „	−0·01
Radcliffe 2	1858·2			55·55	5 „	+0·05
Yarnall	1861·4			55·63	2 „	−0·02
Harvard College	1865·			55·73	8 „	+0·04
The formula compared is		15	12	56·022 + 0ˢ·0250 (t − 1875).		

I have here omitted the authorities Piazzi, Struve, Taylor, Henderson, and Rümker, which I know only at second hand; they would not alter the result by any large amount. The proper motion as usually given is much less. Argelander has +0ˢ·0084, Main +0ˢ·012, and Mädler +0ˢ·0166; the first two would become nearly correct if the error in Bradley were allowed for. Possibly the error +0ˢ·0166 in Argelander's P.M. would account for a portion of the discrepancy in his R.A. I do not know when the observations were made.

There is no trouble with the declination; the annual P.M. is nearly −0″·534 according to Mädler.

Williams College,
Williamstown, Mass., U.S.A.,
1878, *June* 13.

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